THURSDAY, NOVEMBER 30, 1882

THE INDIAN SURVEY

General Report on the Operations of the Survey of India during 1880-81. 61 pp. Report, 93 pp. Appendix, and 22 Plates. (Calcutta, 1882.)

THIS Report for 1880-81 (the fourth since the various branches of the Indian Survey were amalgamated) shows as usual a good amount of useful work done in the year, and contains also many points of general interest. There were in all twenty-nine field-parties and six large head-quarters' offices. The whole out-turn of work cannot be shortly stated, and the total cost is not given; but it appears that there were 22,765 square miles surveyed topographically, 6141 square miles in great detail, besides much minor and special work, also that eleven Revenue Branch parties surveyed 11,326 square miles at a cost of about 81,000*l*.

The principal triangulation of India proper as designed by Col. Everest, *has now been finished.* The result is shown on a skeleton map, which is itself a wonderful sight. There is a continuous "chain" of triangles right round India proper, connected across by *many* meridional and east to west "chains," the longest being from Mussoorie to Cape Comorin (say 1600 miles north to south), and from Chittagong to Kurrachee (say 1800 miles east to west). Outside India proper there are five important extensions, viz. (1) to Kandahar and Khelat; (2) over Káshmír; (3) up the Indus beyond Leh; (4) up the Brahmaputra to Sudya; and (5) a coast "chain" from Chittagong to Tenasserim. This great work, now finished, is one of which India may well be proud.

Certain important changes of procedure are being introduced in the general survey work, viz.: (1) All the topographical work is being brought to a uniform system; (2) Fieldbooks are gradually giving way to direct plotting of detail in the field with advantage in speed of work and economy; (3) Special riverain surveys will in future be made; (4) Local agency is being tried for detail work; this last measure is expected to effect great economy in the survey of Burma, for which at present a staff of 2500 men is taken from Calcutta and back again each season.

Great difficulties often beset the parties in the wilder parts of the country. Many parts are extremely unhealthy, and the parties often suffer severely from fever, &c. In a few parts the roughness of the country, in others climatic conditions, render travelling exceptionally difficult, *e.g.* travelling in the hot wind across the "Rann" (great salt desert) of Katch is dangerous to both man and beast. In a few parts even in India proper, *e.g.* among the wild Bhíls, the surveyors are looked on with suspicion and sometimes attacked. The greatest praise is due to men who carry out their field-work through such difficulties as these.

In general a survey party now accompanies every military expedition; thus some extension of geographical and trigonometrical work was done in 1881 by parties sent with the Mahsúd Wazírí expedition and with the troops at Kandahár. A curious difficulty has arisen in that the modern use of the heliostat in military signalling almost precludes its use for survey stations along with an army.

Self-registering tide-gauges with 5 feet barrels have been set up at fourteen places, and have worked well as a whole. Tide registrations (mostly from older instruments) for twenty-three years in all have been analysed by the harmonic analysis at enormous labour. The discussion shows (for the first time) the existence of a "lunar fortnightly tide" as had been expected from the tidal theory. Tide-tables for 1882 were published for fifteen ports.

The tidal stations of Madras and Bombay have been connected by "levelling" right across the peninsula with the curious result that the mean sea-level at Madras *appears* to be 3 feet above that of Bombay. The cause of this is still a subject of inquiry. It is really a very curious question. Thus, it is said that "there can be no sensible differences of level," *i.e.* as determined by *levelling*, "because the causes by which they would be produced must equally affect the spirit-levels of the instruments and the water-levels of the ocean," so that had the "levels been carried, without error, along the coast line from Bombay . . . to Madras, they must have shown identity of sea-level, &c." On the other hand it is also said that "the Western Ghâts are a source of attraction, which, if not counteracted, must raise the sea-level at Bombay no less than 31 feet above the mean sea-level at Madras." The difference (which should be zero?) is attributed to *observation-error*, and chiefly to the effect of the oblique sunlight illuminating the two ends of the instrument-bubble unequally: thus it is said that an error of only 1·2 seconds in levelment (a very minute quantity) at even one-fourth of the instrumental stations would produce the total error in question.

An interesting improvement has been introduced in the engraving branch, viz. in steel-facing the copper-plates, and is said to be very successful. Apparently engraving on copper is still largely used (as also in the British Ordnance Survey), but it would seem that this tedious and costly process must give way to some of the rapid and cheap photographic processes. The Indian Survey is also utilising the latter very largely, with the wonderful result that "at present publication may, and frequently does, follow the survey in a few days." There is a curious instance of the possible saving in departmental manufacture, in that about 334*l*. has been saved by making up collodion in the office instead of purchasing it.

Of underground temperatures it is noted that at Dehra the maxima occurred at the three depths, 6·4, 12·8, 25·6 feet, about September 20, October 15, and November 15 respectively (the maximum in the air being probably in June?).

An extraordinary outburst of solar spots, covering 630 million square miles, was observed to take place on July 25, 1881, within a period of thirty-seven minutes; it is rare that so grand an outburst is so closely located in time.

The Indian Survey was well represented at the Venice Geographical Exhibition. The whole collection sent seems to have excited great interest, especially the tidal instruments which were connected with the Main Canal so as to be shown in actual work. This exhibition brought to light a striking difference in recent practice of construction of instruments in England and on the Continent in that recent improvements in graduating circles are so

great as to lead to the general adoption (by continental makers) of small circles with powerful reading micrometers in place of large circles with verniers.

A very simple process of making relief maps in Germany is described, viz. by cutting out contour strips from a contoured map, and pasting each on to cardboard cut to same outline. Altogether the Report is a very interesting one.

ALLAN CUNNINGHAM

GREEN'S "GEOLOGY"

Geology. By A. H. Green. Part I. Physical Geology. Third and Enlarged Edition. (London: Rivingtons, 1882.)

STUDENTS of Geology will welcome this third and much enlarged edition of Prof. Green's excellent text-book, though they may at first sight regret the exchange of the old convenient manual form of the book for that of the present handsome and well-printed octavo. One of the first features that strikes the reader in this new issue of the work is the large augmentations made to the lithological sections. In fact this part of the treatise may be said to have been re-cast and almost wholly re-written. The author devotes 150 closely printed pages to crystallography and the description of minerals. It may be open to question whether the full details which he gives to the crystallographic characters of minerals are not rather out of place in a geological treatise. They are not ample enough for the mineralogical student, and the geologist who takes up the subject must necessarily study text-books of mineralogy, where they are given at much greater length. Prof. Green, however, has put them so clearly and succinctly that this portion of his book cannot fail to be of use.

Some changes have been made in the arrangement of rocks. The non-crystalline or derivative rocks now come first—a grouping which no doubt has its advantages in teaching, particularly in elementary classes, but which is not that usually employed in petrographical works After briefly describing the lithological character of the non-crystalline rocks, the author, following his original plan, proceeds to discuss the mode of formation of these rocks, dealing first with denuding agents and their work, and then considering the manner in which the denuded material is aggregated into rock-masses. In these sections he brings his subject abreast of the onward march of the science. Another change in the original treatment of his subject occurs in the author's chapter on the "confusedly crystalline rocks." He has not been so happy in his choice of a title for them as he has been in his description of their general characters. After giving an account of the lithological features he proceeds to discuss their modes of origin, dealing first with volcanoes recent and extinct, then with earthquakes (though one wonders what these have to do with a description of crystalline rocks), next with plutonic rocks which, however, are rather inadequately discussed. The chapter on metamorphic rocks has been carefully revised, and may be commended to the student as an admirable summary of what is at present known on this subject. The chapter upon the way in which rocks came into their present positions was one of the best in the first edition of the book. Its excellence has now been increased by a thorough revision. For practical insight into the structure of the earth's crust it is unsurpassed in any treatise known to us.

Prof. Green more than makes up for the curious omission in the first edition of any mention of mineral veins. We doubt, however, the advantage of inserting minute descriptions of metallic ores in a general geological text-book. The author would do well in his next edition to give references to the Continental works on mining, particularly to some of the numerous treatises which have been published in Germany. Chapter XIII. retains its place as a valuable account of how the present surface of the ground has been produced. The last two chapters discuss the former fluidity and present condition of the earth's interior, the cause of upheaval, contortion, and metamorphism, and the origin of the changes of climate which have taken place during geological time. These parts of the book are exceedingly well done. The author has held the balance fairly between contending disputants, and sums up the evidence with conspicuous and judicial impartiality. Altogether, he may be congratulated on the appearance of this edition of his text-book, which sustains and extends his reputation as an exponent of his favourite science.

OUR BOOK SHELF

A History of British Birds. By the late William Yarrell. Fourth Edition, Revised to the end of the Wryneck, by Alfred Newton, M.A., F.R.S., continued by Howard Saunders, F.L.S., F.Z.S. Part XV. (London: John Van Voorst, November, 1882.)

THE fifteenth part of the new edition of what the British ornithologist fondly calls his "Yarrell" contains the final contribution of Prof. Newton to this work, and the first pages of the portion which Mr. Howard Saunders, his successor in the editorship, has undertaken. Few of the subscribers, we believe, will be much pleased with the change of authorship of their favourite work of reference. No living writer, it may be confidently asserted, is so competent to prepare a new edition of "Yarrell's British Birds" as Prof. Newton, and the conscientious care with which he has laboured upon the two volumes now completed must be patent to all who consult them. At the same time it should not be forgotten that time is an element in all human matters not even excepting books on British birds. When, therefore it is considered that nearly eleven years have elapsed since Prof. Newton commenced his new edition, and that only the first half of the work is now completed, it is obvious that the Professor has not acted unwisely in surrendering the second half to an editor who is able to devote more time to the undertaking.

Mr. Howard Saunders, it is generally understood, intends to issue the two final volumes of the new edition in two years, and if his health and strength permit, will doubtless accomplish his task within the allotted period. In this his large practical knowledge of the bird-life of Southern Europe, as well as his well-known familiarity with modern ornithological literature, are likely to be of the greatest assistance.

Mr. Saunders commences his second volume with the pigeons, and gives us an excellent account of the four British species, as also of the American passenger pigeon, which can only be looked upon as one of our rarest stragglers from the New World. When, however, he says that all true pigeons lay two eggs he must have forgotten that the crowned pigeons, and the numerous forms of fruit-pigeons are, so far as is known, content to lay but one. There is therefore no good reason for calling the

Columbæ "Bipositores," as one of our systematists has proposed to do! After the pigeons Mr. Saunders places the sand-grouse as an intermediate order between the Columbæ and Gallinæ. This is certainly a better plan than that adopted by some of the more ardent reformers of the ornithic system—of uniting the sand-grouse in the same group with the pigeons, and thus spoiling the symmetry of the order Columbæ. In this and in other particulars the new Editor of "Yarrell's Birds" show a judicious spirit, which cannot fail to make the results of his labours generally acceptable.

Episodes in the Life of an Indian Chaplain. By a Retired Chaplain. (London: Sampson Low, Marston, Searle, and Rivington, 1882.)

THIS interesting narrative of the adventures and vicissitudes of a devoted and single-minded Indian Chaplain, appears to be addressed to two classes of readers. A considerable portion must be considered more or less theological, and hence not applicable to the columns of NATURE; but running throughout the unambitious work is a considerable residue of facts and observations relating to zoology, which are never tiresome and sometimes original. In the days of his boyhood our author's leisure time was given to his "different collections of natural history and antiquities," and after many years' official duties he seems to have once more resumed his early tastes, on his appointment to the curatorship of the museum and secretaryship of the public gardens belonging to the Maharajah of Travancore. It is whilst employing his leisure in this vocation that the reader experiences more of the naturalist and less of the chaplain, but both phases are so kindly and modestly described, as to disarm criticism and at the same time promote an amiable impression of the writer.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Sir George Airy on the Forth Bridge

SIR GEORGE AIRY'S letter (*vide* NATURE, vol. xxvi. p. 598) criticising Messrs. Fowler and Baker's design for the Forth Bridge is so important, that I think it but right, as I am not without experience on the subject, to make some remarks on the subject of it. Sir George Airy states:—

1. "That the proposed construction is, as applied to railway bridges, entirely novel." This is not quite exact. There are a number of cantilever bridges in America; and I have, myself, used practically similar principles of construction and erection, on a large scale, with entire success, and find them so satisfactory that, for a very long span, I would not think of using any other.

2. "The magnitude of its parts is enormous." Undoubtedly they are—and all the more credit to the men who had the nerve to design them.

3. "There has been no succession of instances of the construction with rising degrees of magnitude which might furnish experimental knowledge of some of the risks of construction." If this reason were sound, the same objection would have prevented the construction of the Conway, Britannia, and Saltash bridges, and *Great Eastern* steamer; but so far from the statement being correct, the engineering profession has gained ample experience in the erection of the St. Louis, Kentucky River, Douro and Minnehaha bridges to give assurance that the Forth Bridge can be made a perfect success.

4. "The safety of the bridge depends entirely on a system of end thrusts upon very long rods." This is a very singular statement. What would become of the safety of the bridge in case there was no answering and complementary tension system equally exposed to danger from a "system of end pulls upon very long rods" does not appear from Sir George's letter; nor does he seem to remember that the tests of the last few years show conclusively, that iron exposed to compression within its buckling limit is compacted in texture and strengthened by such use while, if subjected to continuous tension beyond two-thirds of its elastic limit, it is attenuated and weakened.

5. "No reference is made to theory applied to the buckling of rods under end thrusts." None was necessary. Mr. Baker has designed struts, or columns—not rods. These members in the Forth Bridge are presumed to have such a proportion of diameter to length that the question of buckling does not come into consideration. In America, columns of many shapes—in full-sized sections—have been tested in lengths of from 10 to 70 diameters, and the value of these shapes, in pounds of resistance per square inch of section for each $\frac{\text{length}}{\text{diameter}}$ is definitely known.

These results are now the common property of all English-speaking engineers. Sir George Airy's remarks on long struts are the more extraordinary, as there is in England, in the upper chord of the Saltash Bridge, an example of a long strut without lateral support which is greater in its ratio of length to diameter than any member that I know of in the Forth design. Moreover, it is 455 feet long, near enough to the length of St. Paul's Cathedral for him to contemplate in connection with that edifice, in presenting a picture to the people of London.

6. "The liability to ruinous disturbance by the lateral power of the wind acting with the leverage of the long brackets appears to be alarmingly great." This liability to destruction by wind is common to all large spans; but the danger is greater in the case of a suspension bridge than in any other (I speak with some knowledge on this point, having made the effects of tornadoes a special study for a number of years past, and having visited most of the bridge wrecks which have occurred in the States, from this cause, since 1858). So far as destruction by wind can be guarded against in the Forth design, it has apparently been done; and the bridge will be vastly stronger in this regard than many other bridges in England which can be easily named, and about the strength of which there is supposed to be no question.

To conclude:—The opinion of those American engineers with whom I have conversed on the subject, and whose experience in building long-span bridges makes that opinion valuable, is uniformly to the effect that the design of Messrs. Fowler and Baker is well digested, perfectly practicable as to execution, and thoroughly permanent in character when finished.

I may also add that three years since, when called on to design a railway bridge for the crossing of the Great Colorado cañon, which was to be 900 feet span and 750 feet above the river, I investigated the relative merits and cost of the various systems—arch-suspension and cantilever with mid-span. Working drawings were made of each, and the result was, that the cantilever was adopted as being equally strong and stable—less liable to be affected by wind and thermal changes, aud decidedly more economical in first cost and easier of erection than either of the others. I am, therefore, not surprised that the engineers of the Forth Bridge should have reached the same conclusion.

CHARLES SHALER SMITH

St. Louis, Mo., November 11

The Aurora

I HAD not the good fortune to see the very unusual phenomena which took place during the aurora of Nov. 17. It was, however, well seen by four of the students of this College, Messrs. Sykes, Wildeblood, Thornhill and Wackrill. Although you are doubtless inundated with letters on the subject, I send a short account of the observation, as such an opportunity of determining the height of an auroral light very rarely occurs. The commencement of the movement of the "Whitehead-torpedo-shaped" streak of light does not appear to have been noticed by them; it passed however just below the moon, one observer thinks that its upper edge just grazed the lower edge of the moon. The light when close to the horizon bore due south-west, a position which has since been verified by bearings taken by a prismatic compass. The spot where the observers stood is, by the new ordnance map in lat. 51° 25′ 57″ N., and long. 0° 34′ 5″ W.

HERBERT MCLEOD

Royal Indian Engineering College, Cooper's Hill, Nov. 24

AT Ilford, Essex, on the 17th instant, at 6h. 4m. p.m. by a watch which was within 2m. of G. M. T., I witnessed, during

the auroral display, the extremely singular phenomenon which has been described by several of your correspondents. It looked exactly like a white cloud, about 20° long and 2° wide, tapered somewhat from the middle to each end; but it was more luminous than a cloud could well have been at that time. When first seen, its nearest end may have been 30° east of the moon. Its length was nearly parallel to the horizon, and continued so till lost sight of about as much to the west of the moon; and its passage over an area of some 80° occupied probably less than a minute. It passed very near to the moon, but I cannot say whether over it or not. CHARLES J. TAYLOR

Toppesfield Rectory, Halstead, Essex, Nov. 25

FOLLOWING up my last week's letter concerning the electric meteoroid, if one may so term it, of the 17th inst., I have sifted all the testimony within my knowledge, assigning a numerical weight to each report from internal evidence of its probable value, and correcting for latitude where the altitude of the moon was made the standard of comparison. With data so precarious, and triangles so ill-conditioned, the results can of course only be regarded as a very rough approximation to the truth; for what they are worth, however, they are as follows:—1. That the course of the meteoroid was about S. 70° W. Probably it was 71° 45′, the complement of the magnetic declination. 2. That there was a proper motion of a little more than a mile a minute. 3. That the path was vertically over a line upon the earth's surface, whose least distance from Greenwich was 72 miles. 4. That the actual elevation was 44 miles. On this reckoning the body would seem to have crossed in the zenith in North Belgium, the Boulogne district, Cherbourg, and the north coasts of Brittany. STEPHEN H. SAXBY

East Clevedon Vicarage, Somerset, November 28

MY observation at Ramsbury, near Hungerford, was to the effect that while watching the northern aurora, my attention was called, at ten minutes past six, to this monster meteor, then slowly approaching in a direct line to the moon, which was shining most brilliantly. It seemed to pass exactly over the disc, and reappeared on the side, much reduced in size, as if going away from us; and at a distance of about 6° from the moon, scarcely seemed to measure more than 5° in length, it being then about 6h. 8m., which corresponds with the position over Sidmouth at that time. It was very definite in form, like a torpedo. I estimated its length at 15°, and 3° in breadth. I hope to have a hand-made photograph of its appearance ready for publication, by the Autotype Company, in a few days, and on the same sheet is a hand-delineation of the great comet to the same scale. ALFRED BATSON

The Rookery, Ramsbury

Lavoisier, Priestley, and the Discovery of Oxygen

In the last number of this journal my friend Mr. Tomlinson has criticised my observations on the respective claims of Lavoisier and Priestley to the discovery of oxygen. Without examining, or attempting to refute one of my arguments, and without the citation of any warrant, or authority, he has stated his opinions with an asseveration worthy of a 15th century Professor of Dogmatic Theology. His letter consists of *five* general statements, and *nine* dogmatic assertions. I have endeavoured to show that of the former, *two* are self-evident truths, or at least universally-admitted conclusions, while the remaining *three* are misstatements; and that of the latter *five* are completely erroneous, while *three* are open to question, and *one* is correct.

1. The universally admitted conclusions are:—(*a*) that "chemistry has no nationality," and that "discoverers are mutually dependent." Nothing that I have said can possibly be construed into the expression of a shadow of doubt concerning the truth of either of these statements.

2. The three misstatements are that (*a*) I have "thought it necessary to revive the old oxygen quarrel," (*b*) that I have "taken an unpatriotic part against Priestley," and (*c*) "endorsed the complacent statement of Wurtz, that chemistry is a French science founded by Lavoisier." If it be reviving a quarrel and acting an unpatriotic part against a man, to show that by the light of evidence hitherto overlooked one of the greatest scientific men of the last century has been unfairly accused of dishonesty, I am quite willing to be considered unpatriotic and a quarrel-monger. As to endorsing the statement of M. Wurtz, all I say is that he did not say it "without reason." Many people regard the assertion as quite unreasonable. I confess I do not, but at the same time I do not mean to say that I fully accept it.

[As to my "forgetting, perhaps, that the title 'La Chimie Française' was invented by Fourcroy, and objected to by Lavoisier," I may say that I do not see that this bears the least upon the question. Lavoisier's own words are "Cette théorie n'est donc pas, comme je l'entends dire la théorie des chimistes français, elle est *la mienne*, et c'est une propriété que je réclame auprès de mes contemporains et de la postérité." (*Œuvres de Lavoisier*, tome 2, 1862, p. 104.) Dr. Thomas Thomson (*Hist. of Chem.* p. 101, vol. ii.) says, "Lavoisier's objection, then, to the phrase *La Chimie Française*, is not without reason, the term *Lavoisierian Chemistry* should undoubtedly be substituted for it." But this does not affect the question whether or no chemistry is a French science as M. Wurtz puts it, for surely Lavoisier was a Frenchman of the French. I say nothing, however, as to the justification of the remark that chemistry *is* a French science.]

3. "That the compound is always equal to the sum of its elements was known long before Lavoisier" remarks Mr. Tomlinson. I have nowhere asserted that it was not, but the statement is new to me, and I should like to have references.

4. . . . "So early as 1630 Rey gave the true explanation of the increase of the weight of metals by calcination." Any one who will take the trouble to read through Rey's essay "*sur la recherche de la cause pour laquelle l'estain et le plomb augmentent de poids quand on les calcine*," cannot fail to observe how very vague his ideas on the subject were. He indeed attributed the increase of weight to thickened air (*l'air espessi*), but the following, as I have elsewhere stated, seems to have been his mode of reasoning:—Air possesses weight; it may be produced by heating water, which during distillation separates into a heavier and a lighter part; hence as air approximates to a liquid nature, it may be supposed to be separated into a heavier and a lighter part by the action of heat; now the heavier part (the "dregs") of air is more nearly allied to a liquid than air, for it has assumed a "viscid grossness," and this part attaches itself to calces during the process of calcination, and causes such of them as possess much ash to be heavier than before calcination. If we calcine a vegetable or animal substance there is no gain of weight, because the assimilated thickened air weighs less than the volatile matter expelled by heat; but in the case of a metal the assimilated air weighs more than the volatile matter expelled, hence there is a gain of weight. Thus he imagined that all calces, from a vegetable ash to a metallic calx, attract this thickened air. It can scarcely be said that a man with these extremely crude notions "gave the true explanation of the increase of weight of metals by calcination."

5 and 6. "Lavoisier's note of 1772 was, as he admitted, based upon Priestley's earlier experiments, begun in 1744." I can nowhere find in Lavoisier's writings any admission of the kind alluded to. (Will Mr. Tomlinson give references?). On the other hand, I do find a note by Lavoisier at the end of Chap. VI. *De la calcination des metaux*, published in the *Opuscules Physiques et Chimiques* (1774), (*Œuvres*, Vol. I., p. 621), in which he says, "Je n'avais point connaissance des experiences de M. Priestley, lorsque je me suis occupé de celles rapportées dans ce chapitre. Il a observé, comme moi et avant moi, . . . &c., &c." This would seem to sufficiently disprove the former statement.

Mr. Tomlinson speaks of Priestley's "earlier experiments begun in 1744." Now Priestley was born in 1733, and although no doubt a clever fellow he certainly did not begin to experiment at *eleven* years of age! His first paper on gases was published thirty-nine years later, viz. in 1772.

7. That "the acceptance of Lavoisier's doctrine was mainly due to the capital discovery of the composition of water by Cavendish in 1784," I utterly deny; and if desirable will show cause why. Nevertheless, as it has been so asserted, we may, for the present at least, regard it as an open question.

8. Mr. Tomlinson calls Black, Priestley, and Cavendish, "the founders of pneumatic chemistry." Surely John Mayor and Stephen Hales have a better right to the title.

9. "Priestley discovered oxygen in 1774." This, no doubt, is true in a sense because everybody says so. If it means that he got a gas from red oxide of mercury it is true. But let us not forget:—(*a*) that he discovered it by a random experiment, "by

accident" as he confesses; (*b*) that he regarded it as air containing nitrous particles; (*c*) that he remained in complete ignorance of its nature till March, 1775, before which time Lavoisier was well acquainted with its principal properties, and had recognised many of its functions.

10. "Cavendish discovered hydrogen in 1784." On the contrary, he described it in his "Experiments on Fictitious Air," published in 1766.

11. "Davy abjured Lavoisier's *principe oxygène*, and by his numerous discoveries gave the chemical edifice so rude a shake that it had to be taken down and rebuilt." From our point of view, *in spite of* the numerous discoveries of Davy, the edifice erected by Lavoisier, and which is still standing, had not to be taken down and rebuilt, except in one small part. The theory of acidification was a small part of Lavoisier's labours, and it was Berthollet who called chlorine *oxymuriatic acid*, and who thought that he had proved it to be a compound of muriatic acid and oxygen.

12. Mr. Tomlinson after asserting that "chemistry has no nationality," and "that discoverers are mutually dependent," goes on to say with strange inconsistency that chemistry "had no proper existence for us until Dalton discovered its laws." Surely this is almost as if he slightly altered the "complacent statement of Wurz," and said, "Chemistry is an English science; it was founded by Dalton of immortal memory." We do not think that many will differ from us when we say that chemistry was a science long before the time of Dalton.

Thus we have endeavoured to show that of the nine dogmatic assertions given above (numbered 4-12):—*one*, viz. 9, is correct; *three*, viz. 7, 8, and 11, are open to grave question; while *five*, viz. 4, 5, 6, 10, 12, are altogether erroneous.

There is no possible excuse for us to remain any longer in ignorance of the mighty works done by Lavoisier. The fine quarto volumes, 1862-1868, published by the French government, are a fitting monument to the genius of the man. The petty jealousies which disfigure the history of science during the end of the last, and commencement of the present century, ought to find no place in our minds. The Republic of Science is large enough for every man to receive his due.

G. F. RODWELL

The Comet

IT would scarcely perhaps be civil to take no notice of Mr. Backhouse's letter in NATURE, vol. xxvii. p. 52, the object of which seems to be principally to discredit my account of the disappearance of the comet in a moonlit sky. Still less, however, would it be reasonable to take offence at it—albeit, Mr. Backhouse is wrong. Indeed, a little more reflection might have shown him that ample time having elapsed without any correction from me appearing in your columns, the presumption must have been strong that I had nothing to correct. I have in fact seen the comet frequently since—as well as many times before—and am moreover really experienced enough not to have made quite so gross a blunder; or at least to have found it out, if I did make it, when so many subsequent opportunities permitted. Besides that, I have fortunately the following testimony in corroboration. One of my sisters wrote, "What you did not see of the comet agrees exactly with F.'s experience. She looked out at Court-Lodge: splendid night; many, even small, stars, though moon shining bright; but the comet *wasn't to be seen*, though she and Miss B. scanned the whole fine expanse of east and southeast sky." Another wrote about the same time that though visible two days later, it was so pale that she did not wake a nephew who wished to see it. My drawing of the 23rd October has two stars above the nucleus, with one of which it made the base of an isosceles triangle, the other being at the vertex. These two stars were plainly visible all the morning of the 30th, but not so high above the roof across the way, but what the motion of the comet since I last saw it (23rd) *may* have lowered it enough to conceal the nucleus. In fact, either I am wholly right as to the disappearance, nucleus and all, under moonlight, or at least the nucleus must have been concealed. There is no other alternative. As to the great sweep of tail—let us be reasonable in our guesses as to the fallibilty of others however improbable their evidence. May not something for instance be ascribed to the London atmosphere as likely to increase the amount of moonlight reflected? It was for this that I wished the observation made public, viz. as a real phenomenon having a real cause; all the more interesting that it was so surprising—nay, as it seems, so incredible. My only regret is that I have been now tempted into so long a reply.

Before I leave the comet, may I presume to express my surprise that the question as to this comet's return is still *sub judice*. It is said that three well observed places are enough to determine the elements of a comet's orbit. But this one has surely furnished more nearly a score since its perihelion, to say nothing of those before—which no doubt belong to a previous orbit. It is not without fear that I may be misunderstood, that I ask of those who are skilled in such things for an explanation, knowing that of all men they are most deeply interested in the early solution of such a question. It may be said that the observations at and about the time of perihelion have scarcely yet reached this country; but is not the fact that the comet was at one time, which I imagine is known with some certainty, *behind the sun's disc*, equivalent to an observation of its place sufficiently exact to rank with others in calculating the orbit? I do not presume to say that it is so. I merely formulate a question which, in its general bearing, must surely be agitating the minds of many besides myself, after all we have read about the possible past history and future fate of this remarkable comet. It has now been under observation during two months, in which time it must have traversed nearly one quarter of its entire orbit, if an elliptical one of moderate extension. Its present path in space must be so nearly straight that continued observation can hardly be expected to furnish improved data until, if ever, departure from that shall settle the question decisively in favour of an elliptical path. But is it for this that we must wait? I can hardly think so, for surely no comet has ever yet been *seen* in the neigbourhood of aphelion.

J. HERSCHEL

30, Sackville Street, November 18

An Urgent Need in Anthropology

BOTH zoology and geology possess a yearly "record" of the work achieved in their respective domains, but anthropology still remains without that aid to its proper advancement. All workers are of course cognisant of the current bibliography given in the German anthropological publications, and the supplemental information on the same subject contributed by Dr. O. Mason in the *American Naturalist*, and are not unappreciative of the same; but these lists are but partial, and necessarily incomplete, as must be evident when the peculiar nature and wide scope of the study of man is taken into consideration.

Compared with anthropology, the record of zoological work is simple in the extreme. Zoology possesses its accredited organs and regular channels of publication, and with trifling exceptions, its yearly work can be gleaned from these sources. But what is anthropology? It may be described as the very Talmud of humanity with its "Mishnah" of ethnological facts, and its "Gemara" of anthropological conclusions. Scattered up and down the bye-ways of literature, here and there recorded by the traveller, illustrated by the historian or accentuated by the essayist, hidden in blue-books, and awaiting extraction from medical reports, existing in the papers of the missionary and the publications of the statisticians are the unaccumulated and unrecorded facts and observations which form the foundation on which to rear a complete science of man. Our own savages afford as excellent illustrations of the comparative in civilisation as do the primitive peoples of the jungle or the swamp, and hence a large fund of information is still to be supplied and tabulated from our city alleys, prisons, and lunatic asylums. To the question, Is such a record needed? must be added, How is such a record possible?

It seems at once clearly impossible that such a work could be either intrusted to the care of one man, or to the men of one nationality. No individual can be expected to have perused the whole current literature of his country, and could such a phenomenon be discovered, it is still more unlikely that he would combine in himself those qualities which are necessary to detect the varied data that make for anthropology. An alternative course, however, is present, which is possible, and not too exhaustive as regards time and labour. In each country where anthropology is cultivated as a science, a few of its votaries could form an association for the purpose of abstracting from *its* literature such facts, arguments, and observations as appertain to the study of man, and these might, in a condensed and tabulated form, appear as a regular yearly contribution in the pages of the different publications of the varied ethnological and anthropological societies which now embrace so many nationalities. It is perhaps not presumptuous to say that these papers would not be the least valuable in the volumes in which they appeared. It seems work that anthropological societies might

justly undertake, and we might then expect to hear less of the little interest felt in the science by the general public. When we have an "applied anthropology" to our daily life, and a system of anthropology taught in our public schools, we shall wonder how it was that the science so long remained in the esoteric stage. However, paradoxical as it may seem for the writer to admit, no science has been illustrated by so many excellent handbooks and compendiums as anthropology. From the time of Prichard to the works of Lubbock, Peschel, and Tylor, there have always been competent workers and writers, and the last-named works represent the very essence of our knowledge on the subject. In the face of this there is still a vast and unrecognised mass of material waiting extraction from the total annual literature of each country.

One other work requires compilation, and refers to the past. How frequently a traveller or missionary, anxious to write fully on the people he has visited, and wishing at the same time to have his views enlarged by the opinions of others, inquires for the list of authors and authorities who have written on the same subject. With very few exceptions such a desideratum is unprocurable, and yet if we would at present understand the social position of any tribe, however degraded or improved, the records of their earliest visitors must be compared with the narratives of their latest describers. This again can only be the work of a specialist, who, having carefully searched for and studied the literature relating to some particular tribe or race, would voluntarily present his "bibliography" to students at large, and for that purpose endeavour to have the same published by his local or some other anthropological society. These lists, if once begun, would slowly accumulate, and would not only confer lasting fame on their compilers, but also, by their publication in the *Transactions* of the societies devoted to the study of man, make the contents of those works more valuable by their presence, and at the same time promote the absence of some memoirs which a further knowledge of the subject would render somewhat unnecessary.

It is, however, only in the hope of further suggestions from other workers, that I have ventured to obtrude these remarks in the columns of NATURE. W. L. DISTANT

A Modification of the Gold-leaf Electroscope and a Mode of Regulating its Charge

THOSE who experimentalise with the usual form of gold-leaf electroscope must know well that the instrument requires a vast amount of preparation and drying before it is ready for use, and also that in wet weather it keeps its charge but a little while. At the same time the electroscope when in good order is a beautifully sensitive instrument and of great value in demonstration. I have made a slight addition to the present form of instrument, which makes it useful in all states of the weather. A flat spiral is cut out of sheet ebonite with a fret saw, about 8 mm. wide, and 4 mm. thick; the diameter of the spiral is the same as the internal diameter of the glass shade; the spiral is cemented to the shade just below the line at which its dome begins; the centre of the spiral carries the brass rod to which the gold leaves are attached; the rod comes up through the top of the shade without touching it; thus a very long insulator is placed between the charged leaves and the surface of the shade; on a damp day the leaves are powerfully divergent two to three hours after being charged. If instead of the spiral a little tube of ebonite takes the place of the usual varnish glass tube, the charge will be kept a fairly long while.

If the same angle of divergence of the gold leaves be required in two similar electroscopes, charged, say, with electricity of opposite sign, this can be effected by fully charging each instrument, and then bringing a lighted candle about ten centimetres above the brass disc or knob of each; by lowering or raising the candle, the charge can be drawn off as slowly as you please. It is well known that a flame has been used ATTACHED to an electrometer in testing atmospheric electricity. Volta used a flame connected to an exploring rod, and in Sir W. Thomson's electrometer a slow-burning match is used; but it will be noticed that in the experiment I have described for regulating the charge, the flame is only held near the disc or knob, but is NOT allowed to TOUCH IT. I also find, and it is very remarkable, that electroscopes can be fully charged by placing them about a metre from a charged jar, if a taper be now placed on the top of the jar, by means of an insulator the leaves instantly diverge and the electroscopes remain charged. FREDERICK JOHN SMITH

Taunton, November 18

Palæolithic Gravels

THE subject of the preservation of human remains in drift beds has been so fully discussed by every author who has written on the antiquity of man, that it would be mere waste of space to reprint what has been so many times printed before. No doubt the day will one day arrive when we shall have plenty of examples of the osseous framework of palæolithic man; at present but few of his bones have been found for study. Human bones are extremely liable to decay, but no doubt some of our palæolithic precursors are preserved somewhere; they will be lighted on some day.

In 1878 I had an opportunity of removing the stones from several cairns at Cynwil Gaio, in Carmarthenshire; the kistvaens or stone graves were then exposed. On carefully removing the covering stones from each kist, the place in which the human body was originally deposited was laid bare. The soft, smooth bed of fine clay (brought from a distance) was there on which the body was placed at the time of burial, but not in a single instance was there a trace of a bone, a tooth, or any relic whatever of the body; it had entirely vanished. Now if we can find nothing in a grave that is only a very few thousands of years old, what can we expect from one that is tens or possibly hundreds of thousands?

When Prof. T. McK. Hughes lectured on the Antiquity of Man before the Victoria Institute he said (reprint p. 8): "I will not waste time to discuss whether the objects we refer to man, now found in numbers in post-glacial river-gravels, are really of human work." The Professor was quite right, for any one who can see any art in the Parthenon, or any human work in Raphael's Cartoons, ought to see art in palæolithic implements; and, of its class, uncommonly good art too. But none are so blind as those who won't see, and many persons have not strength of mind or courage enough to accept the teachings of their own reason. WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, North

Ancient Monuments

WHILST in North Wales last autumn, I visited the famous Kist-Vaen, on Tynycoed Farm, Capel Garmon, not far from Bettws-y-coed. This is a sort of double subterranean cromlech, the single cap-stone now remaining being on a level with the ground. On two of the large upright supporting stones, two blockheads had painted their names in green oil-paint from top to bottom of the stones. The trouble of taking the green paint and brushes to this place must have been considerable, and I hope now that General Pitt-Rivers is appointed Inspector of Ancient Monuments, he will find these parties out, and make them take a pailful of turpentine, and rub out the offensive inscriptions.

I also visited the two circles of stones, termed on the Ordnance Map Maenan-hirion, by Penmaenmawr, and looked out for the two outlying stones stated to be on the north-east side of the larger circle. I could not see them; there is a large naturally-imbedded boulder on the east-south-east side, but the intermediate one has been removed. Whilst I was at the smaller circle, I noticed that one of the stones had recently been pulled out of its setting, and was lying beside the hole.

The great camp on Penmaenmawr was plentifully bestrewn with sandwich papers and empty bottles, but the immense walls and hut circles of our forefathers defy the efforts of excursionists to a great extent. I, however, saw several of these terrible persons on the top, taking off the stones from the ancient walls and throwing them down below.

I noticed several other stones in the neighbourhood of the circles that had recently been thrown over.

In some of the more romantic and rocky situations in Wales—places visited by "cheap trips" (as near Bettws)—the rocks and even highly-esteemed antiquities—as the elaborately carved roadside cross at Carew, Pembrokeshire—are plastered over with printed bills about auctions, tea-meetings, sermons, and quack medicine. WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, N.

Shadows after Sunset

IN reference to Mr. Douglas Archibald's letter, I may say that in 1873 I made three drawings of the "Sheaf rays" at the Isle of Wight. In these they are marked as "con-

verging in the east," but the point is apparently below the visible horizon. Shortly after I had, however, the opportunity of seeing the true convergence, as we were crossing the Peasemarsh, a large common near here. It was after rain, and there appeared a very bright spot in the east opposite the true sun, which to the best of my recollection was setting and not set, for I momentarily took the appearance to be some form of reflection of the sun itself. The rays were quite strong in the east and west, and though fainter could be distinctly traced across the sky. I believe that there were no clouds and that the ray intervals were equidistant, though I will not be certain on this point. I notice that one of my drawings also shows this peculi rity, though I confesss my impression has been hitherto that these rays were due to the interference of clouds. J. RAND CAPRON

Guildown, Guildford, Nov. 24

On the Isomerism of Albuminous Bodies

AMONG organic compounds there are large number of bodies hoving the same composition, but different constitution. They are called isomerides. The number of these isomerides increases in proportion as the number of atoms which they contain increases.

Prof. Cayley has already calculated the possible number of isomerides of hydrocarbons. From his result it can be easily seen that the increase of isomerides in proportion to the complexity of the composition is an exceedingly rapid one.

Now the number of atoms which the so-called albuminous bodies contain are very large. The number of isomerides which they can give therefore must be exceedingly large, in fact almost innumerable.

Prof. Schorlemmer, in his "Rise and Development of Organic Chemistry," says: "The enigma of life can only be solved by the synthesis of albuminous compounds." If then these albuminous bodies are really the basis of life, the different species of living beings must come from innumerable sources, for albuminous bodies have innumerable isomerides. According to this theory, we can say that the different species of living beings, whether animals or plants, were developed out of the chemical compounds having the same composition, but different constitution, but cannot assert, as some do, that they were developed out of the same source, or a few sources

Tokio, Japan, October 12 SHIGETAKÉ SAGIURA

An Extraordinary Lunar Halo

ON Monday evening, November 20, an unusual halo surrounded the moon from 6.15 to 6.25. The moon was not quite full, and the halo to some extent assumed the form of the moon. The halo consisted of a succession of concentric rings. The ring next the moon was equal to four diameters of the moon, and had a soft yellow-white radiance, almost equalling the moon in brilliancy; it was surrounded by a succession of prismatic rings, red commencement, and proceeding outward orange, yellow, green, blue, indigo, and violet. At 6.15 the chromatic rings were pretty sharply defined, with the exception of the outer one, which was faint and evanescent. Outside of the ring was a corona-like envelope. This aspect continued about five minutes, and during the next five minutes rapidly changed; the edges of the rings became irregular, radii shot from the rings towards the moon, and at 6.25 the phenomenon disappeared.

Newcastle-on-Tyne, November 24 J. P. BARKAS

Meteor

A BRIGHT meteor was seen here about 4.30 p.m. in the east. It did not explode, but dissipated itself with scintillations. It reached a very low level before it disappeared.

Oxford, November 27 W. L. HARNETT

Flame in Coal Fire

THE flame referred to by Major Herschel (NATURE, vol. xxvii. p. 78) is simply that of carbon monoxide, which may be observed in most coal fires, after the hydrocarbons are consumed, burning with a pale blue flame. Any yellow tint is of course due to sodium present in the coal. The production of carbon monoxide depends more upon the arrangement of, than the quality of, the coal. Major Herschel will find the reason of its presence give in any text-book on chemistry.

I cannot understand what advantage is obtained by removing the slit of the spectroscope, especially if one wishes to show that a flame is mono-chromatic. When burnt at ordinary pressure, carbon-monoxide has no definite spectrum. SM.

Rugby, November 24

Waterspouts on Land

I AM of opinion that the phenomena referred to by Mr. Hossack are not the effect of waterspouts, but are rather to be attributed to landslips. I may mention a case which may throw some light on the matter. About 1872 (I cannot give the exact date) a landslip occurred on the banks of the Tay, about seven miles north of Dunkeld, close to Guay Station on the Highland Railway, and on the east side of that line. I lived close by at the time, and shortly afterwards saw the effects. Local opinion attributed it to the following causes:—Along the top of the gravelly slope planted with oak and other trees, ran a brook. Immediately above the place where the landslip occurred, the banks of the brook had been burrowed by rabbits. When the sudden flood occurred which caused the landslip, the water of the brook entered these holes, undermined the gravelly slope or terraced beach, and precipitated it across the highway into the field below, devastating fully an acre of it. The trees, turf, &c., were deposited in the field much as they grew upon the slope. I was surprised that they had not been overturned, but it would appear that they had slid down. The effects are still quite visible to passengers on the railway. Had they been photographed at the time, they would have formed a capital illustration for a geological text-book.

Guildhall Offices, Carlisle JOHN GEDDES MCINTOSH

NOTES FROM THE LETTERS OF CAPTAIN DAWSON, R.A., IN COMMAND OF THE BRITISH CIRCUMPOLAR EXPEDITION

MAY 21. *On board the s.s. "Nova Scotian."*—A grey sky, a grey foam-flecked sea, floating ice-floes, fog and rain, with a thermometer a few degrees above freezing—such are the features of the Gulf of St. Lawrence this morning, and a cheerful welcome to the New World. Our course has been a long way to the south of Newfoundland on account of the ice, consequently our passage has been a long one. Yesterday was quite lovely, several icebergs were in sight eight or ten miles off, looking like peaks of snow mountains at a distance; now we are in the midst of ice fields delaying us a good deal, as at times it is difficult to find a passage.

May 22. *Quebec.*—We sighted land last night, and saw such a lovely sunset as we went up the St. Lawrence. We have been steaming up the river eighteen hours, but we cannot yet see the land on both sides. We have just passed the *Peruvian*, which left Liverpool a fortnight before us, but she got among the ice and broke her screw, and has been twenty-seven days on the voyage. Another of the Allan line steamers ran into an iceberg. So we feel lucky in getting across without mishap. At the end of the week I start for Winnipeg—2,500 miles by rail—a long journey of five days and four nights.

I find Quebec quite wintry after England; indeed, the snow is still lying in sheltered places where it has drifted, and no trees are in leaf.

June 3. *On Lake Huron.*—On reaching Toronto we went back again into summer—everything was green and spring-like, and the air was quite soft and balmy.

We left Toronto for Sarnia, where we embarked for Duluth, on the west end of Lake Superior—thence it is about twenty-four hours' journey to Winnipeg. Toronto was looking very well. There are groves of horse-chestnut trees in the principal streets, which have a very good effect. At Toronto I was introduced to the Canadian Premier, who took a great interest in my expedition. I also dined with the chief of the observatory there, and they gave me some wine at dinner which was made from their own vines in the suburbs. To Sarnia is about six hours—a most fertile country. The weather, however, is very rainy at present—this is the wet time of the year.

Every train and steamer is full of young Englishmen on their way out to Winnipeg, where they expect to make their fortunes, and no doubt it is a great place to make money just now.

We were two days running up Lake Huron, for the most part out of sight of land, and the land we did see was flat and ugly, till in the evening of the second day we reached the river joining the two lakes (Lake Huron and Lake Superior), and anchored at Sault St. Marie for the night. The river runs between rocky pine-covered shores, and in the evening we had one of those sunsets one only seems to see in this country—a blood-red sky overhead, orange at the horizon, with the pine woods rising black against it, and the broad reaches of the river winding away westward, all a blaze of golden light—just the subject for Turner.

The next morning we went through a lock into Lake Superior, and twenty-six hours' run took us to Thunder Bay, a barren looking place. Hard by is Thunder Cape, a bold headland 1,300 feet high. The water of Lake Superior never rises more than two or three degrees above freezing-point, and in old days it would certainly have been thought an enchanted lake, so strange are the effects of the mirage. At one moment you see a long line of cliffs, a minute later they have turned into a reef of rocks hardly above the water, or a little table-topped mountain on the horizon suddenly splits into two sharp peaks, and anon takes the shape of an hour-glass.

June 8. *Winnipeg.*—When we awoke this morning we were on the prairies—just like the sea, only grass instead of water—a green plain losing itself in the far horizon. The journey along the Northern Pacific Railway, from Duluth, by the side of the rapid river St. Louis was lovely.

Winnipeg is a flourishing place with 20,000 inhabitants, where a few years ago there was nothing but a few huts. It stands on the Red River of the north—a fine river about the size of the Rhine. All the people here are Cree Indians, who speak their language and don't understand English, but they are dressed in European dress, so they look more like gipsies than anything else.

June 27. *Fort Carlton.*—We arrived here yesterday, such lovely country, like an English park, with wild roses and other flowers growing in great profusion. The river rather reminds me of the Thames at Richmond. The Saskatchewan is a magnificent stream, far larger than the Red River, flowing between pine forests. The weather is simply perfect, except that the sun is rather hot in the middle of the day. The fare is rather rough. No milk or fresh bread—chiefly fish, biscuit, and salt meat. It was slow work getting up the rapids. A boat with a crew of Indians takes out a hawser a mile long, which is made fast to a tree above the rapids, then the other end is brought down to the steamer, and fastened to the capstan, and we slowly drag ourselves up. The steamer is propelled by an enormous paddle wheel at her stern, and at the bow is a great arrangement of spars for lifting her off sandbanks, should she run aground; and though she carried 150 tons of cargo she only drew three feet of water.

On the 23rd we reached the Forks of the Saskatchewan, where the river divides; we took the northern branch and warped up the rapids to the settlement of "Prince Albert," where the country looks quite like England. Land is to be had here for 2 dollars or 8*s*. an acre, and it seems wonderfully fertile—the soil looks so rich. It is certainly the place I should recommend any enterprising emigrant to come to if he only has a little capital to start with—300*l*. would be plenty. The soil wants no clearing; you have only to build a house and plough and sow your land. The climate is one of the finest in the world. I was talking to a retired officer of the 50th who has been here seven years, who says he has never had an hour's illness, and feels as though he were growing younger every year.

Fort Carlton is the *beau idéal* of a Hudson's Bay fort, with a stockade twenty feet high and towers at the corners. But the days when the Blackfeet made their raids are over, and the Cree or Ojibbeway Indians, whose "lodges" one sees all around, are very pacific. A great many speak a little French, but no English.

July 14. *Ile à la Crosse.*—We left Carlton on the 30th, *i.e.* my own party and two missionaries. I had a train of ten Red River carts drawn by horses and oxen. I drove in a light American waggon. The scenery was at first like English country, only without hedges. There was plenty of deep grass and vetches, which afforded splendid fodder for the animals. There were quantities of snipe, duck, and prairie chicken. The land was gay with wild flowers; orange-lilies were most conspicuous, and lots of wild strawberries. The mosquitoes were the only drawback, at times forcing us to wear veils and gloves, and to eat our meals in the smoke of our camp fires. After three days we reached a hill, from whence we saw the great sub-arctic forest stretching away like a sea to the north. It extends nearly to the Arctic Circle, and from the Atlantic to the Pacific.

On the 9th we reached Green Lake, but it blew so hard that we did not start till the 11th. Our conveyance was a Hudson's Bay Company's inland boat; our crew was of Crees and Chipewyans. The latter speak a language like the ancient Mexicans, quite unlike any other I have heard; it is like the noise of a person choking. It takes years to learn even a smattering of it. We drifted down the stream all night, our boats being lashed together, and we slept as best we could in the bottom of them.

Ile à la Crosse is on an island in the middle of a lake, and is comparatively free from mosquitoes. I had a splendid boat's crew—seven oars and a steersman; we pulled nearly fifty miles the first day. We rested on Sunday, and the day after crossed Buffalo Lake most fortunately—a fair wind sprang up just in time to take us across, as it cannot be crossed against the wind. Then we began to ascend the Rivière la Loche, which took us all the next day, there being two portages or places where the contents of the boats and sometimes the boats themselves have to be taken overland. Thence we entered Methy Lake, about thirty miles long at the north, and a narrow creek took us to the beginning of Portage la Loche, or the Long Portage, which is a road some twelve or fourteen miles long, leading to the Clear Water River which flows into the Athabasca and ultimately into the Mackenzie, so we are on the Arctic Slope at last.

July 22. *Portage la Loche.*—I rode over last night in company with the Hudson's Bay Company's officer in charge of the post. The road leads through pine woods, and passes a pretty lake, and ultimately descends a hill of about 400 feet into this valley. I am writing this in my tent, pitched on the bank of the Clear Water River, which flows past about three yards off. Across the river are wooded hills 600 feet high; to the left the river disappears among the pine woods in a dark ravine; to the right it winds away in the distance among blue hills. It is all so green and pretty that it is difficult to believe that in a few months all will be ice and snow. All the last week the heat has been intense, the thermometer over 86° in the shade all day. This morning we saw a bear prowling about opposite. We are now among the Chipewyan Indians; they are very different from the Crees; in appearance they remind me a little of drawings of the Esquimaux, with round greasy faces. About here they are mostly Roman Catholics, as there is a large mission at Ile à la Crosse.

The best description of this country in general is by saying that it is like Switzerland without mountains, but with big rivers and lakes. The plants are much the same, and the climate is much the same. The trees are very fine, and, as elsewhere, strawberries, raspberries, cranberries, black and red currants, and gooseberries grow wild.

There is a fine view down the valley from the top of the hill; it was mentioned by Sir J. Frankland, who has been through all this country.

July 24.—The Athabasca boats arrived last night, so we are off this morning.

ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE[1]

II.

In the preceding investigation nothing has been said as to the units in which the quantities m and H are measured. It will be convenient, before proceeding further, to consider shortly the measurement of magnetic and electrical quantities in absolute units, and particularly the centimetre, gramme, second (c.g s.) system now generally adopted.

According to what is called the electro-magnetic system, all magnetic and electrical quantities are measured by units which are derived from a magnetic pole chosen as the pole of unit strength. This pole might be defined in many ways; but in order to avoid the fluctuations to which most arbitrary standards would be subject, and to give a convenient system in which work done in the displacements of magnets or conductors, relatively to magnets or to conductors carrying currents, may be estimated without the introduction of arbitrary and inconvenient numerical factors, it is connected by definition with the absolute unit of force. It is defined as *that pole which, if placed at unit distance from an equal and similar pole, would be repelled with unit force.* The poles referred to in this definition are purely ideal, for we cannot separate one pole of a magnet from the opposite pole of the same magnet: but we can by proper arrangements obtain an approximate realisation of the definition. Suppose we have two long, thin, straight, steel bars, which are uniformly and longitudinally magnetised; their poles may be taken as at their extremities; in fact, the distribution of magnetism in them is such that the magnetic effect of either bar, at all points external to its own substance, would be perfectly represented by a certain quantity of one kind of imaginary magnetic matter placed at one extremity of the bar, and an equal quantity of the opposite kind of matter placed at the other extremity. We may imagine, then, these two bars placed with their lengths in one line, and their blue poles turned towards one another, and at unit distance apart. If their lengths be very great compared with this unit distance, say 100 or 1000 times as great, their red poles will have no effect on the blue poles comparable with the repulsive action of these on one another. But there will be an inductive action between the two blue poles which will tend to diminish their mutual repulsive force, and this we cannot in practice get rid of. The magnitude of this inductive effect is, however, less for hard steel than for soft steel, and we may therefore imagine the steel of our magnets so hard that the action of one on the other does not appreciably affect the distribution of magnetism in either. If, then, the two blue poles repel one another with a unit of force, each according to the definition has unit strength.

The magnitude of unit pole is by the above definition made to depend on unit force. Now unit force is defined, according to the system of measurement of forces founded on Newton's Second Law of Motion, the most convenient system, as that force which, acting for unit of time on unit of mass, will give to that mass unit of velocity. Our unit pole is thus based on the three fundamental units of length, mass, and time. According to the recommendations of the B.A. Committee, and the resolutions of the Paris Congress, it has been resolved to adopt generally the three fundamental units already in very extended use for the expression of dynamical, electrical, and magnetic quantities, namely, the centimetre as unit of length, the gramme as unit of mass, and the second as unit of time. With these units, therefore, the unit force is that force which, acting for one second on a gramme of matter, generates a velocity of one centimetre per second. This unit of force has been called a *dyne*. The unit magnetic pole, therefore, in the c.g.s. system of units is that pole which, placed at a distance of 1 centimetre from an equal and similar pole, is repelled with a force of 1 dyne. Each of the poles of the long thin magnets of our example above is therefore a pole of strength equal to one c.g.s. unit, if the mutual force between the poles is 1 dyne.

The magnetic moment m of anyone of the deflecting magnets is equal to the strength of either pole multiplied into the distance between them, which for magnets of such great length in comparison with their thickness is nearly enough the actual length of the magnet. Therefore either pole has a strength of $\frac{m}{2l}$ units. If r and l are measured in centimetres, and W in grammes, the strengths of the magnetic poles deduced from equation (4) or (6) will be in c.g.s. units.

A magnetic field is the space surrounding a magnet or a system of magnets, or a system of conductors carrying currents, at any point of which, if a magnetic pole were placed, it would be acted on by force. From the definition of unit magnetic pole we get at once the definition of magnetic field of unit intensity. *Unit magnetic field is that field in which unit magnetic pole is acted on by unit force*, and in the c.g.s. system, therefore, it is that field in which unit magnetic pole is acted on by a force of one dyne. In the theory of the determination of H, given above, the horizontal force on either pole of the needle due to the horizontal component of the earth's field is taken as $\frac{m'}{2l'}.H$, and again the horizontal force on either pole of the deflecting magnet as $\frac{m}{2l}.H$. H is, therefore, the strength in units of magnetic field intensity of the horizontal component of the earth's field. By formula (5) or (7), when r and l are taken in centimetres, and W in grammes, H is given in dynes; that is, it is the number of dynes with which a unit red pole would be pulled towards the north, and a unit blue pole towards the south if acted on only by the earth's magnetic field. We can now go on to the measurement of currents.

According to the theory of electro-magnetic action given by Ampère, every element of a conductor in which a current is flowing acts upon a magnetic pole with a force which varies inversely as the square of the length of the line joining the centre of the element with the pole, and directly as the strength of the current and as the length of the projection of the element on a plane at right angles to that line. The direction of this force is at right angles to a plane drawn through the pole and the element, and acts towards one side or the other of that plane, according as the current in the element is in one or the opposite direction, and according as the magnetism of the pole is red or blue. From this it is easy to obtain a definition of unit current in the electromagnetic system. It is that current which, flowing in a wire of unit length bent into an arc of a circle of unit radius, acts on a unit magnetic pole placed at the centre of the circle with unit force. Thus the current of unit strength in the complete circle of unit radius would act on a unit pole at the centre with 2π units of force, in the c.g.s. system with 2π dynes. This force acts towards one side or the other of the plane of the circle, according to the nature of the pole and the direction of the current. If the current, considered as flowing from the copper plate to the zinc plate of a Daniell's cell, were made to circulate round the face of a watch in the direction opposite to that in which the hands move, a red pole placed at

[1] Continued from p. 35.

the centre would be moved out through the face of the watch, and a blue pole in the opposite direction; and the opposite would be the case if the current were reversed. This is easily remembered by those familiar with the representation of couples in dynamics, by observing that when the direction of the current is the same as that in which a positive couple tends to turn a body, the direction in which a red pole is urged is that in which the axis of the couple is drawn. Or, the direction of the force may be found at any time, by remembering that the earth may be imagined to be a magnet turned into position by the action of a current flowing round the magnetic equator in the direction of the sun's apparent motion.

From the definition of a magnetic field we see that unit current may also be defined as that current which, flowing in a wire of unit length bent into an arc of a circle of unit radius, produces at the centre of the circle a magnetic field of unit intensity. The direction of the resultant magnetic force at that point is by Ampère's law at right angles to the plane of the circle, and the side towards which it acts in any particular case may be found as stated above.

If we take then the simple case of a single wire bent round into a circle and fixed in the magnetic meridian, with a magnet, whose dimensions are very small in comparison with the radius of the wire, hung by a torsionless fibre so as to rest horizontally with its centre at the centre of the circle, we may suppose that each pole of the magnet is at the same distance from all the elements of the wire. A current flowing in the wire acts, by Ampère's theory, with a force on one pole of the needle towards one side of the plane of the circle, and on the other pole with an equal force toward the other side of that plane. The needle is thus acted on by a couple tending to turn it round, and it is deflected from its position of equilibrium until this couple is balanced by the return couple due to H. Let us suppose the strength of each pole of the needle to be m units, r the radius of the circle, and C the strength of the current in it. Then by Ampère's law we have for the whole force without regard to sign, exerted on either pole of the needle by the current, the value $Cm\frac{2\pi r}{r^2}$ or $Cm\frac{2\pi}{r}$. If l be the length of the needle the couple is $Cm\frac{2\pi}{r}l$, before any deflection has taken place. After the needle has been deflected through the angle θ the arm l of the couple has become $l\cos\theta$, and therefore the couple $Cm\frac{2\pi}{r}l\cos\theta$; and the return couple due to H is $mHl\sin\theta$. Hence we have equilibrium when

$$Cm\frac{2\pi}{r}l\cos\theta = mHl\sin\theta$$

and therefore

$$C = \frac{Hr}{2\pi}\tan\theta \quad . \quad . \quad . \quad . \quad . \quad (8)$$

if θ be the observed angle at which the needle rests in equilibrium when deflected as described from the magnetic meridian. If instead of a single circular turn of wire we had N turns occupying an annular space of mean radius r, and of dimensions of cross-section small compared with r we should have

$$C = \frac{Hr}{2\pi N}\tan\theta \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In practice the turns of wire of the tangent galvanometer may not be all contained within such an annular space. It is necessary then to allow for the dimensions of the space occupied by the wire. For a coil made of wire of small section we may suppose that the actual current flowing across a unit of area is everywhere the same. Hence if C be the current strength in each turn, and n the number of turns in unit area, we have for the current crossing the area A of an element E the value $2\pi nCA$. Taking a section of the coil through the centre, let BC be a radius drawn from the centre C in the plane cutting the coil into two equal and similar coils, and taking $CD(=x)$ and $DE(=y)$ at right angles to one another, we have $A = dx\,dy$ and $CE^2 = x^2+y^2$. Hence the force exerted on a unit magnetic pole at the centre C by the ring supposed at right angles to the plane of the paper, of which this element is the section, will be $\frac{2\pi nCy\,dx\,dy}{x^2+y^2}$ in the direction at right angles to CE and in the plane of the paper. If we call the component of this force at right angles to BC, dF, we have

$$dF = \frac{2\pi nCy^2\,dx\,dy}{(x^2+y^2)^{\frac{3}{2}}}.$$

Hence for the whole force at right angles to BC we have

$$F = 2\pi nC\int_{-b}^{b}\int_{r-c}^{r+c}\frac{y^2\,dx\,dy}{(x^2+y^2)^{\frac{3}{2}}},$$

where r is the mean radius of the coil, $2b$ its breadth, and $2c$ its depth in the plane of the circle.

Integrating, and putting N for the whole number of turns $4nbc$, we get

$$F = \pi NC\frac{1}{c}\log\frac{r+c+\sqrt{(r+c)^2+b^2}}{r-c+\sqrt{(r-c)^2+b^2}} \quad . \quad (10)$$

If θ be the angle at which the deflecting couple is equilibrated by the return couple due to H, we have as before the equation

$$F = H\tan\theta.$$

Hence, substituting the above value for F and solving for C, we have finally

$$C = \frac{H\tan\theta}{\pi N\frac{1}{c}\log\frac{r+c+\sqrt{(r+c)^2+b^2}}{r-c+\sqrt{(r-c)^2+b^2}}} \quad . \quad (11)$$

When the value of r is great in comparison with b and c this reduces to the equation

$$C = \frac{Hr\tan\theta}{2\pi N} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

which we found before by assuming all the turns to be contained in a small annular space of radius r. In practice, in galvanometers used as standards for absolute measurements, generally neither b nor c is so great as $\frac{1}{10}$ of r, and in these cases the difference between the values given by equations (11) and (12) is well within the limits of errors of observation, and the correction need not be made. The value of C given by (12) is then to be used.

In this investigation the suspension fibre has been supposed torsionless. If a single fibre of unspun silk is used as described below for this purpose, its torsion may for most practical purposes be safely neglected. The error produced by it may however be easily determined and allowed for by turning the needle, supposed initially in the magnetic meridian, once or more times completely round, and noting its deviation from the magnetic meridian in its new position of equilibrium. The amount of this deviation, if any, may be easily observed by means of the attached index and divided circle, or reflected beam of light and scale, used as described below, to measure the deflections of the needle. From the result of this experiment the effect of torsion for any deflection may be calculated in the following manner.

Let α be the angular deflection, in radian[1] measure, of the magnet from the magnetic meridian produced by turning the magnet once round, then the angle through which the thread has been twisted is $2\pi-\alpha$. The couple produced by this torsion has for moment $Hlm\sin\alpha$.

[1] A *radian* is the angle subtended at the centre of a circle by an arc equal in length to the radius. It has generally been called in books on trigonometry hitherto by the ambiguous name *unit angle in circular measure*.

Hence, by Coulomb's law of the proportionality of the force of torsion to the twist given, we have for the couple corresponding to a deflection θ the value

$$\frac{\theta}{2\pi - a} H m l \sin a.$$

If then under the action of a current in the coil the deflection of the needle is θ, the equation of equilibrium is

$$C m \frac{2\pi}{r} l \cos\theta = m H l \left(\sin\theta + \frac{\theta}{2\pi - a}\sin a\right)$$

and therefore instead of (9) we have

$$C = \left(1 + \frac{\theta}{2\pi - a}\frac{\sin a}{\sin\theta}\right)\frac{Hr}{2\pi N}\tan\theta . \quad (13)$$

If a be an angle of say 1°, and θ be 45°, $\frac{\theta}{2\pi - a}$ is very nearly $\frac{1}{8}$ and $\frac{\sin a}{\sin\theta}$ is $\frac{1}{57{\cdot}3} \times \frac{1}{{\cdot}707}$ or $\frac{1}{40{\cdot}5}$. Hence

$$C = \left(1 + \frac{1}{324}\right)\frac{Hr}{2\pi N}.$$

The error therefore is somewhat less than $\frac{1}{3}$ per cent.

The accuracy of the measurements of currents, made according to the method of which I have just given the theory, of course altogether depends on the careful adjustment of the standard galvanometer, and the care and skill of the observer. The standard galvanometer should be of such a form that the values of its indications can be easily calculated from the dimensions and number of turns of wire in the coil. Such a galvanometer can be made by any one who can turn or can get turned a wooden, or, preferably, brass ring with a rectangular groove round its outer edge to receive the wire. It is indeed to be preferred that the experimenter should at least perform the winding of the coil and the adjustments of the needle, &c., himself, to make sure that errors in counting the number of turns or in determining the length of the wire, or in placing the needle at the centre of the coil, are not made. The breadth and depth of this groove ought to be small in comparison with its radius, and each should not be greater than $\frac{1}{10}$ of the mean radius of the coil, which should be at least 15 cms. The size of the wire with which the coil is to be wound must be conditioned of course by the purposes to which the instrument is to be applied, but it should be good well insulated copper wire of high conductivity, and not so thin as to run any risk of being injured by the strongest currents likely to be sent through the instrument. For the exact graduation of current as well as potential galvanometers directly by means of the standard instrument, it is convenient sometimes to have two coils—one of comparatively high, the other of low resistance. The latter may very conveniently be a simple hoop of say 15 cms. radius, made of copper strip 1 cm. broad and 1 mm. thick. To form electrodes to which wires can be attached the ends of the strip are brought out side by side in the plane of the ring with a piece of thin vulcanite or paper between for insulator. Insulated wires are soldered to the ends of the circle thus arranged, and are twisted together for a sufficient distance to prevent any direct effect on the needle from being produced by a current flowing in them. In constructing a coil the operator should first subject the wire to a considerable stretching force, and then carefully measure its electrical resistance and its length. He should then wind it on a moderately large bobbin, and again measure its resistance. If the second measurement differs materially from the first the wire is faulty and should be carefully examined. If no evident fault can be found on the removal of which the discrepance disappears, the wire must be laid aside and another substituted. When the two measurements are found to agree the wire may then be wound on the coil. For this purpose the ring may either be turned slowly round in a lathe or on a spindle so as to draw off the wire from the bobbin, also mounted so as to be free to turn round. The wire must be laid on evenly in layers in the groove, and the winding ended with the completion of a layer. Great care must be taken to count accurately the number of turns laid on. The resistance should now be again tested, and if it agrees nearly with the former measurements the coil may be relied on. The ring carrying the coil thus made should now be fixed to a convenient stand in such a manner that if necessary it can be easily removed. The stand should be fitted with levelling screws so that the plane of the coil may be made accurately vertical. A shallow horizontal box with a glass cover and mirror bottom should be carried by the stand at the level of its centre. Within this the needle and attached index are to be suspended. The needle should be a single small magnet about a centimetre long, hung by a single fibre of unspun silk about 10 cms. long from the top of a tube fixed to the cover of the shallow box, so that the centre of the needle when the coil is vertical is exactly at the centre of the

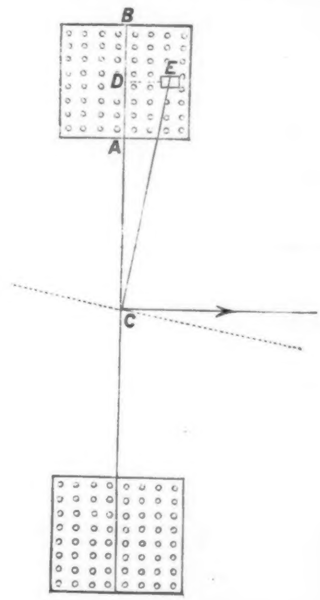


Fig. 3.

coil. To allow of the exact adjustment of the height of the needle, the fibre should be attached to the lower end of a small screw spindle, made so as to be raised or lowered, without being turned round, by a nut working round it above the cap of the tube. The needle should carry a thin glass index, about 6 inches long, made by drawing out a bit of thin glass tube at the blowpipe. In order that the zero position of the index may not be under the coil, the index should be fixed horizontally with its length at right angles to the needle, so as to project to an equal distance in both sides of it. To test that this adjustment is accurately made, draw a couple of lines accurately at right angles to one another on a sheet of paper. Then suspend a long thin straight magnet over the paper, and bring one of the lines into accurate parallelism with it. Remove then the magnet and put in its place the little needle and attached index. If the index is parallel to the other line the adjustment has been carefully made. The needle may then be suspended in position and the box within which it hangs closed to prevent disturbance from currents of air.

A circular scale graduated to degrees, with its centre just below the centre of the coil and its plane horizontal, is placed with its zero point on a line drawn on the mirror bottom of the box at right angles to the plane of the coil, so that when the needle and coil are in the magnetic meridian the index may point to zero. The accuracy of the adjustment of the zero point is to be tested by finding whether the same current produces equal deflections on the two sides of zero. To test whether the centre of this divided circle is accurately under the centre of the needle supposed at the centre of the coil, draw from the point immediately under the centre of the needle two radial lines on the mirror bottom, one on each side of the zero point and 45° from it, and turn the needle round without giving it any motion of translation. If the index lies along these two radial lines when its point is at the corresponding division on the circle the adjustment is correct.

When taking readings the observer places his eye so as to see the index just cover its image in the mirror bottom of the box, and reads off the number of degrees and fraction of a degree, indicated on the scale by the position of the index. Error from parallax is thus avoided.

A mirror with attached magnets may be used, as in the magnetometer, instead of the needle and index. When this arrangement is employed the coil is in the magnetic meridian, when equal deflections of the spot of light on the scale on the two sides of zero are observed. These scales, as has been already remarked, should always be carefully glued to a wooden, piece instead of being, as they frequently are, fixed with drawing pins.[1]

ANDREW GRAY

(To be continued.)

PROFESSOR HENRY DRAPER, M.D.

THE late Professor Henry Draper, whose death we announced last week, was born in Virginia in 1837, but three years later removed to New York, at the time when his father, Prof. J. W. Draper, was appointed to the Chair of Chemistry in New York University. At this University Dr. Draper was educated, graduating in Medicine in 1858, after which he travelled abroad. In 1860 he was elected to a professorship in his own University, which he retained till his death the other day. In 1866 he was elected Professor of Physiology in the Medical Department of the University and managing officer of the institution, a position he resigned in 1873.

Dr. Draper's scientific work began with a series of experiments in 1857 on the function of the spleen, carried out by the aid of microscopic photography, an art then in its infancy. On his return from Europe, stimulated by a visit paid to Lord Rosse's 6-foot reflector, he began the construction of a $15\frac{1}{2}$-inch reflecting telescope, and with this, when completed, he took photographs of the moon. A full account of the methods of grinding and polishing reflecting mirrors and the system of testing them was printed in 1864 in the Smithsonian "Contributions to Science."

Dr. Henry Draper subsequently constructed an equatorial reflecting telescope of 28 inches aperture, making both the mounting and the silvered glass speculum himself. The object for which this instrument was intended, and which it succeeded in accomplishing in 1872, was photographing the spectra of the stars, a work which has been carried on with such success by Dr. Huggins in this country. Since the invention of the gelatino-bromide dry process the difficulties of this research have much decreased; all the more credit is therefore due to Draper and the other pioneers in this branch of inquiry; he had taken more than a hundred spectra of various stars.

In 1872 Dr. Draper produced a photograph of the diffraction spectrum of great excellence. It comprised the region from below G, wave length 4350, to O, wave length 3440, on one plate.

In 1874 Draper was appointed by the United States Transit of Venus Commission, Superintendent of its Photographic Department, and his duties in this connection were so satisfactorily performed, that in the fall of that year the United States Government caused a special gold medal to be struck in his honour at the Mint in Philadelphia, bearing the inscription, "Decori Decus Addit Avito." This was the first time that such a public recognition had ever been accorded to a scientific man in the United States by the Government.

In 1877 Dr. Draper printed his paper on the "Discovery of Oxygen in the Sun and a New Theory of the Solar Spectrum." This research has given rise to as much interest as any in recent times; whatever the future verdict may be upon it, it was the result of several years' work and most costly and elaborate apparatus. In 1877 Dr. Draper went to the Rocky Mountains, and made experiments on the transparency and steadiness of the atmosphere at elevations up to 11,000 feet. In the succeeding summer he took a party into the same region to observe the total eclipse of the sun, and was fortunate enough to photograph the diffraction spectrum of the solar corona, which on this occasion was shown to be continuous.

During the last autumn and winter he took photographs of the nebula in Orion. These were the first he ever made, and required an exposure in the telescope up to 140 minutes, even when the most sensitive of Eastman's gelatine plates were used.

Dr. Draper's work has been done mainly at his observatory at Hastings-on-Hudson, and at his laboratory in New York. In the former he had three large telescopes.

Dr. Draper's genial nature won him many friends and many English men of science well know the hospitable home at Dobb's Ferry. These and many more will sympathise with Mrs. Draper in the loss which not only she but science has sustained in the death of so earnest a seeker after truth.

THE COMET

WE have received the following communications on this subject:—

The latest information indicates that the September comet was first seen on the 3rd of that month at Auckland.

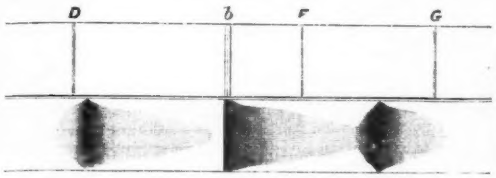
The sketch, No. 1, represents the appearance of the spectrum of this comet on October 15 and 16, and subsequent mornings. The spectroscope used was one of Browning's direct-vision, with five prisms. It was attached to the comet-seeker, which has a 4-inch object-glass, the focal length of the instrument making a distinct general view of the spectrum easy. As the spectroscope was not furnished with any means of comparing spectra, the positions of the bands, as shown in the sketch, were obtained by adjusting the viewing telescope so that each band was, in succession, just in the edge of the field, clamping the telescope, and then viewing the spectrum of a candle. This operation was repeated several times on October 16, and subsequently on the 25th. The position of the band in the orange-yellow was referred directly to the sodium line in the candle-flame. The band in the middle of the green was much the brightest, and on the least refrangible side was sharply defined; but, in the other direction, gradually diminished in brightness. When the slit of the spectroscope was gradually closed, the light was gradually diminished, but no separate line made its appearance, as the well-defined edge of the band would have led one to expect.

The other two bands were of about equal brightness; both of them fading rapidly on the more refrangible side, but much more slowly in the other direction.

[1] ERRATUM.—In the preceding part of this article, p. 30, col. 1, line 24 from top, for 2r read r.

It will be remembered that the first spectroscopic observations, by M. Thollon, at Nice, reported the spectrum of the nucleus as continuous, very brilliant, and much extended toward the violet. The head gave the sodium lines very brilliant, clearly double, and appearing displaced toward the red. This report was confirmed on the same day by a similar one from Mr. Lohse, with the additional remark, that he saw *many bright lines*, the sodium being the brightest, and all apparently displaced toward the red.

October 15. This state of things had entirely changed. The change had probably been gradual, and was dependent upon the distance of the comet from the sun.



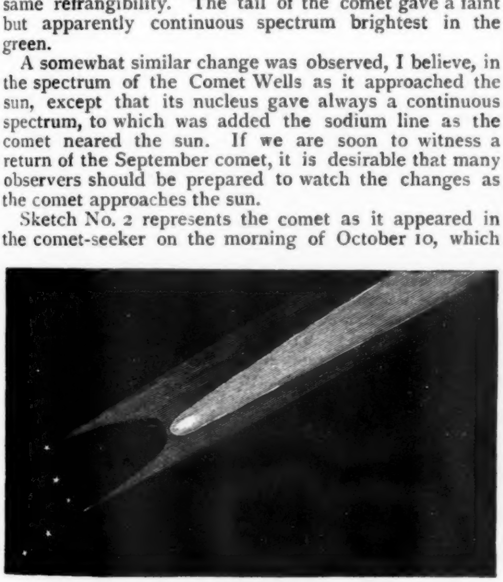
No. 1.—Spectrum of Crul's Comet, October 15 and 16.

The first observation made on September 18, when the comet was near the sun, gave a continuous spectrum, which was due to the strong reflected light, while the bright lines were due to the vapours developed by the intense heat of the sun.

On October 15 the spectrum resembled the spectra of the comets of 1868; of the sodium line there was no trace, although the spectrum contained light of about the same refrangibility. The tail of the comet gave a faint but apparently continuous spectrum brightest in the green.

A somewhat similar change was observed, I believe, in the spectrum of the Comet Wells as it approached the sun, except that its nucleus gave always a continuous spectrum, to which was added the sodium line as the comet neared the sun. If we are soon to witness a return of the September comet, it is desirable that many observers should be prepared to watch the changes as the comet approaches the sun.

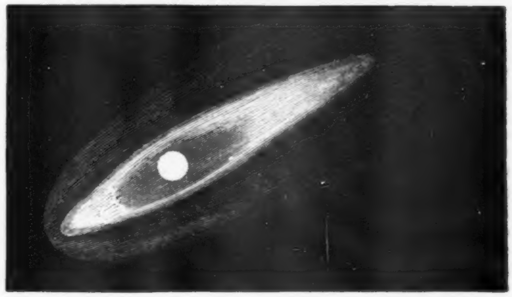
Sketch No. 2 represents the comet as it appeared in the comet-seeker on the morning of October 10, which



No. 2.—October 10, 1882.

was particularly clear. This outer envelope I first noticed on the morning of the 8th, when I traced it far beyond the head of the comet in the direction of the sun, but only on the east side. On the 10th it appeared as represented in the sketch. The outer edges were perfectly sharp and parallel to the axis of the comet, thus forming a cylinder whose diameter was about four times the diameter the tail measured several degrees from the head. When I again looked for this envelope on the 25th, it could be traced only on the east side, but retained the same relation to the tail. The greatest length to which this attained was about four degrees beyond the head.

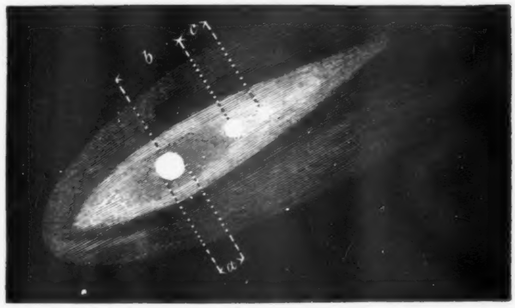
Sketch No. 3 represents the head of the comet as it appeared on October 25 in the 26-inch equatorial. Owing to the low altitude of the comet, this instrument had not been used before. The head appeared more elongated than at any time before but instead of the uncertain, delusive appearance which it presented in the 10-inch equatorial, the image brought out the peculiarities of each portion of the head, and left little doubt that there was, at that time, an essential difference between the central portion or nucleus proper, and the other two portions. The central part was circular, of considerable apparent



No. 3.—Nucleus of Comet, October 25, as seen in 26-inch Equatorial.

diameter, and of quite uniform brightness throughout. The other two portions were irregular in shape, much less bright than the nucleus, and their brightest parts were also irregular in shape. Both were apparently separated from the nucleus, though on its side they were joined to each other. The portion extending in the direction of the tail was the longest and brightest.

The next opportunity to examine the comet was on November 3, when it had decidedly changed in appearance, as represented in No. 4. The nucleus remained as when last seen, as did also the portion of the coma nearest the sun. The other portion showed a circular



No. 4.—Nucleus of Comet, Nov. 6, 1882, as seen in 26-inch Equatorial.

condensation almost as distinct as the original nucleus, and about two-thirds its size; still further in the direction of the tail was another condensation, smaller and less distinct than the second. On November 6 these condensations were still more pronounced, and shone with a much stronger light than the coma in which they were enveloped.

The following micrometric measurements were made on the 3rd and 6th, using the 26 in. equatorial:—

Distance $a = 0''.66$
,, $b = 1.70$ } November 3.
,, $c = 0.84$
Distance $a = 0.74$
,, $b = 1.66$ } November 6.
,, $c = 1.03$

Assuming 0·1705 and 0·1714 to be log. Δ on those days respectively, the distance *a* would be about 4000 miles, the distance *b* about 10000 miles, and the distance *c* about 5500 miles.

It would not be surprising, judging from the history of this comet, if another condensation developed in the portion of the coma nearest the sun, thus forming four nucleuses.

W. T. SAMPSON,
Commander U.S.N.

Naval Observatory, Washington, November 11

Since my first communication, with sketch of the comet, on October 21, which appeared in NATURE, vol. xxvi. p. 622, I have had good views on 21 out of 31 days. The fine weather and clear atmosphere of this place give exceptional facilities for the continued and frequent observations which are needed to obtain a knowledge of so anomalous and surprising an object. Some windows of my villa command an extensive sky and sea view (including at times the mountains of Corsica, 120 miles distant), and from my bedroom—sometimes even from my bed—I have been able to watch the comet with ease for from a quarter of an hour to an hour, on each of those twenty-one days; using only a good field binocular in occasional aid of a strong natural sight. I have more powerful telescopes, but for this object they give no help; and I am not astronomer enough to avail myself of other instruments.

The comet was seen in all its brightness on October 20, 21, 23, and 24, with its nucleus like a star of first magnitude, but elongated and nebulous—its tail beginning with slender stem, slightly curved, with downward convexity, and gradually expanding to its extremity, the diameter of which was about five times that of the head. The lower, slightly convex margin, was brighter, and more defined; but a strong nebulous light pervaded the length and breadth of the tail, shaded along the upper margin in gradually diminishing haze. The tail ended in an elongated crescent, the lower or eastern horn of which was longer than the other. Both horns were prolonged in faint lines, hardly perceptible, a few degrees further (as noticed by your correspondent, Mr. Larden). No such prolongation could be seen from the hollow of the crescent, which terminated by a narrow fringe of diminishing light, beyond which was an oval patch of shade, *obviously darker than any other portion of the visible sky*. This appeared to me nothing else than a *shadow* projected by the comet on the space beyond the end of its tail. I cannot admit the correctness of Major Herschel's *suspicions*, "that this impression was produced by contrast only" (NATURE, vol. xxvii. p. 4). The still greater contrast between the brightness of the lower margin and the adjoining sky produced no such shade there *at that time*: later I shall allude to such a shade appearing there also. The ultra-caudal patch was obviously darker than any other spot of the sky: so it appeared to me, and my experience in landscape painting has given me some skill in appreciating lights and shades. I am quite aware of the difficulty of physically explaining the existence of light and shadow in the vacuity of space, but this is a question of pure observation, to which I invite further attention. Two of your correspondents, Mr. Larden of Cheltenham, and Mr. Cecil of Bournemouth, describe "a black rift in the sky," and "a strong apparent shadow" behind the comet—seemingly in confirmation of my observation.

When the comet was next seen, after an interval of bad weather, on the 29th it had lost in dimensions, but still more in brightness, and its form was changed. The upper margin from the head upwards had expanded and become more feathery; so had the end of the tail, which had lost its crescentic form; the shadow beyond had quite disappeared, and was replaced by an ill-defined luminosity, losing itself in the darkness of the sky. The lower margin of the tail had lost less of its brightness and definition; and now if there was a shadow anywhere, it was along this edge, down even to the head of the comet; but the shade was much less marked than had been that beyond the tail, and I might have ascribed it to contrast but that it was not present when this margin was brighter and the contrast greater. This shadow is noticed by Mr. Cecil in NATURE, vol. xxvii. p. 52.

The comet was well seen on October 30 and 31, and November 2, 3, 4, 6, and 7, gradually diminishing in brightness and in the definition of its outline, its light being now further paled by moonlight. So faint was it that I am not surprised at Major Herschel's description of its non-appearance in the London sky of November 5; but I cannot help "suspecting" that this was due not to moonlight only (as the testimony of others proves), but also to the gas-lit haze of the London atmosphere, which from fifty years' experience I know to be, at its clearest, quite sufficient to mask a faded comet, even although the brighter light of stars may still remain visible. On the 8th the comet was seen before moonrise, more distinct, although pale and hazy in outline; lower margin still the brightest, with a slight attendant shade. It was seen every day (except the 13th, 14th, and 15th) until the 22nd, with little other changes than that it was gradually becoming fainter, although still a conspicuous object in the dark sky from 2 to 5.30 a.m. On the 21st I made a careful portrait of it in oils, with its attendant stars, by the side of one that I had painted from the sketch taken October 21, when it was in its glory. The alteration which has taken place in the month is such that it now seems the mere ghost of its former self. The comparison strikes one as showing how much more it has lost in brightness and compactness, than in length and breadth. Is not this in exact conformity with what has been ascertained (see NATURE, vol. xxvii. p. 58) that the comet has been receding more rapidly from the sun than from the earth.

C. J. B. WILLIAMS

Cannes, November 23

THE APPROACHING ECLIPSE OF MAY 6, 1883[1]

THE sixth of May next year will witness, in the distant regions of Oceania, one of the rarest and most important astronomical phenomena of the century, viz. a total eclipse of the sun, which, owing to the respective positions, but rarely realised, of the sun and the moon, will have a duration quite extraordinary.

Now, in the present state of science, when the most important questions as to the constitution of the sun and that of the unexplored spaces near him, and the existence of those hypothetical planets which Le Verrier's analysis indicated within the orbit of Mercury, are still pending, a phenomenon which presents to us, for long minutes, all those regions, with the sun's dazzling brilliancy withdrawn, and renders them accessible to observation, is one of the first order.

We shall presently examine the conditions under which this rare solar occultation will be produced; let us first see what is the state of the questions which have to be considered on this occasion. One of the most important is that regarding the constitution of the space immediately bordering on the envelopes of the sun at present known.

The great Asiatic eclipse of 1868, came wonderfully *à propos*, both by its long duration and by the maturity of the problems that had to be attacked, enabled us in some sort to tear the veil which hid from us the

[1] Report to the Bureau des Longitudes, by a Commission con[illegible] of MM. Fizeau, Admiral Cloué, Lœwy, and Janssen (reporter).

phenomena existing beyond the visible surface of the sun. It was then that was solved the enigma so long pondered over regarding the nature of those roseate protuberances which surround in such a singular way the limb of the eclipsed sun.

Spectral analysis taught us that they were immense appendages belonging to the sun, and formed almost exclusively of incandescent hydrogen gas. Almost immediately, the method suggested by this same eclipse, and which allows of a daily study of those phenomena, revealed the relations of those protuberances to the solar globe. It was perceived that the protuberances are merely jets, expansions of a layer of gas and vapours, 8″ to 12″ in thickness, where the hydrogen preponderates, and which is at a very high temperature, by reason of its contact with the surface of the sun. This low atmosphere is the seat of frequent eruptions of vapours coming from the solar globe, and among which one chiefly observes sodium, magnesium, and calcium. We may even suppose that, in the lowest part of this *chromosphere*, as it has been called, most of the vapours, which in the solar spectrum, produce the dark lines it presents, exist in the state of high incandescence.

The eclipse of 1869, which was visible in America, allowed indeed of the important observation (always confirmed since) of the reversal of the solar spectrum at the extreme border of the disc, that is to say, at the points where the photosphere is immediately in contact with the chromosphere; a phenomenon which does not signify that the photosphere itself may not contain the same vapours and concur in the production of the solar spectral lines.

Thus the discovery of a new solar envelope, the recognised nature of the protuberances, and the knowledge of their relation to the sun; lastly, the conquest of a method for the daily study of those phenomena; such were the fruits of spectrum analysis applied to the study of this long eclipse of 1868.

But a total eclipse presents other manifestations completely unexplained up to the time of which we speak. There is seen, beyond the protuberances and the chromospheric ring, a magnificent aureole, or luminous corona, of soft brightness and silvery tint, which may reach as far as an entire radius of the dark limb of the moon.

The study of this beautiful phenomenon, by methods which had given such magnificent results, was immediately undertaken, and occupied the astronomers during the eclipses of 1869, 1870, and 1871.

But the aureole or corona, though constituting a brilliant phenomenon, has in reality but weak luminous power. Hence the difficulty of obtaining its spectrum with its true characters. Thus the astronomers differed at first as to the real nature of the phenomenon. In 1871, and by the use of an extremely luminous instrument, it was definitively proved that the spectrum of the corona contains the bright lines of hydrogen, and the green line called 1474 of Kirchhoff's maps, an observation which demonstrates that the corona is a real object constituted of luminous gases forming a third envelope round the solar globe.

If indeed the phenomenon of the corona were a simple phenomenon of reflection or of diffraction, the coronal spectrum would merely be a weakened solar spectrum. On the other hand, the characters of the solar spectrum are here quite subordinate, and the spectrum is that of protuberantial gases and of matter still unknown, indicated by the line 1474.[1]

The subsequent eclipses of 1875 and 1878, and that recently observed in Egypt, have yielded confirmation of these results.

But if the constitution of the sun is being thus rapidly unveiled, there still remain great problems to be solved, both as to this last solar envelope, and as to the region near it.

First of all, have the immense appendices which the corona has presented during some eclipses, an objective reality, and are they a dependance of this immense coronal atmosphere, or might they rather be streams of meteorites circulating round the sun (as one of the members of the Bureau has suggested)?

We do not forget the zodiacal light, the relations of which to those dependances of the sun remains to be determined.

But these problems are not the only ones we have now to attack, during the occultations of the solar globe. Do the regions with which we are occupied contain one or several planets, which the illumination of our atmosphere, so bright in the neighbourhood of the sun, may have always concealed from us? Leverrier long studied this question, and his analytical researches led him to suppose their existence.

On the other hand, several observers have alleged that they have observed transits of round and dark bodies in front of the sun; but these observations are far from being certain. The surface of the sun is often the seat of small, very round spots, which appear and disappear in a time often short enough to simulate the passage of round bodies before that star.

The question is of capital importance; hence it at present justly engages the thoughts of all astronomers.

May the analysis of Leverrier enrich the solar world towards its central regions, as it has done with such a magnificent success in the most distant regions?

We have but two means of solving the problem, whose solution is more particularly incumbent on French astronomy; the attentive study of the solar surface, or the examination of the circumsolar region when an eclipse renders their exploration possible to us. This last means seems the most efficacious, but on the condition that the occultation is long enough to allow of a minute exploration of all the regions where the small star may be met with.

This gives a capital importance to the eclipse of May 6 next, one of the longest of the century.

We will now examine the circumstances of this great eclipse, and the means that it would be well to employ for observation of it.

The total eclipse of May 6 next will have a duration of 6 minutes at the point where the phase is maximum (5m. 59s.); a time triple that of ordinary eclipses.

The central line is wholly comprised in the South Pacific Ocean, and we can only hope to observe it in the islands of that ocean.

After an attentive study of the question, it has appeared to us that two islands would do about equally well for observation; those of Flint and Caroline.

Flint Island (lat. 11° 30′ S., and long. 151° 48′ W. of Greenwich) is the nearest to the central line. Calculation gives for the duration of totality in this island 5m. 33s. Caroline Island is 150° 6′ W., and 9° 50′ S.; the duration of the totality there will be 5m. 20s.; that is, only 13 seconds less than in Flint Island.

It will be seen that the astronomical conditions of the phenomenon are extremely favourable in these islands, and it is to these stations we should propose to the Bureau to send an expedition.

This expedition should start from Paris, go to New York, traverse the American Continent by the railway to San Francisco, and there find a steamer (of a French service about to be established), which should carry it to the Marquesas Islands. There a man-of-war of the French station should take it up, and deposit one portion at Caroline Island, the other at Flint Island. This ship

[1] One of us has expressed the idea (Notice du Bureau des Longitudes, 1879) that the coronal atmosphere which is in dependence on the chromosphere and photosphere must present a much more agitated appearance at the epoch of maximum of spots and protuberances, since the jets of hydrogen which then penetrate it are much more numerous and rich Ulterior observations, and especially those which have been made during the last eclipse at the moment when the solar eruptions were abundant, have confirmed this prevision.

which, further, should be provided with all that is necessary for the establishment of the stations, the safety and the subsistence of the observers, should not leave those regions before bringing the mission to Tahiti, where our *envoyés* would find means of transport for their return, either by the way they went, or (which would seem preferable), by way of Australia.

THE TRANSIT OF VENUS ON WEDNESDAY, DECEMBER 6

AT the Transit of Venus in 1874 the tables of the planet prepared by Prof. Hill appeared to have a decided advantage over those of Leverrier. The correction to the tabular place deduced from the observations of the transit is in close accordance with that shown by a meridian observation at Washington on the day preceding the phenomenon. Although the entire discordance was not negatived by the tables of Prof. Hill, they went far towards removing it in 1874, and as the coming transit (December 6) will take place in nearly the same point in the planet's orbit, we shall assume in what follows, that the tables of the American astronomer will again be fairly correct. Prof. Newcomb assumes and probably with much reason, that the error of Leverrier's tables will prove to be an increasing one, and is therefore inclined to apply a still larger correction to the place deduced from them. It may be mentioned that the calculations of the transit in the *Nautical Almanac*, the *Connaissance des Temps*, and the *Berliner Astronomisches Jahrbuch*, depend upon Leverrier's tables. For the diameter of the planet we adopt that found by Prof. Auwers from heliometric measures in Egypt during the last transit, combining it with the diameter of the sun, inferred by Leverrier from his discussion of the transits of Mercury.

Direct calculations for Greenwich, Edinburgh, and Dublin give with the elements so obtained the following *Greenwich* mean times of the first external contact, and the respective angles from the sun's vertex for direct image:—

	h.	m.	s.	°	′
Greenwich	2	0	42·2	126	59·4
Edinburgh	2	1	7·7	130	49·3
Dublin	2	1	9·6	131	21·2

For a limited area like that of these islands we may apply to these times and angles, the method of distribution of predictions given by Littrow, and subsequently by Woolhouse. Putting the latitude of any place within the above area $= 50° + L$, and its longitude in minutes of time $= M$, $+$ if *east* of Greenwich, $-$ if *west*, we get the following equations:—

G.M.T. of first external contact ... $\Big\} = 2\text{h. } 0{\cdot}62\text{m.} + [8{\cdot}7453]\ L - [8{\cdot}1402]\ M.$

Angle from sun's vertex, direct image... $\Big\} = 126°{\cdot}3 + [9{\cdot}669]\ L - [9{\cdot}136]\ M.$

The quantities within the square brackets are logarithms, but of course if preferred the factors for L and M may be expressed as numbers. As an example of the application of these formulæ, suppose the time of first contact and the corresponding angle are required for Norwich, the position of which place may be taken in latitude $+52° 38'$, longitude $1° 18'$ or $+ 5$m. 12s., we have then

$L = + 2°{\cdot}633 \quad M = + 5{\cdot}20\text{m.}$

	+ 8·7453		− 8·1402
Log. L ...	+ 0·4205	Log. M ...	+ 0·7160
	+ 9·1658		− 8·8562
No.	+ 0·146	No.	− 0·072
	− 0·072		
	2 0·62		
	2 0·69 or 2 h. 0 m. 41 s. G.M.T.		
	Longitude E 5 12		
	2 5 53 Norwich M.T.		

For the angle—

+ 9·669	− 9·136	+ 1·23
Log. L + 0·421	Log. M + 0·716	− 0·71
		126·3
+ 10·090	− 9·852	126·8 ... angle from vertex.

So that according to the calculation the limb of the planet comes into first contact with that of the sun at 53° from his lowermost point towards the left, as we view the phenomenon with the naked eye. It will be remarked that there is less than a half minute difference in absolute time between Greenwich and Dublin, and considering the possibility of error of many seconds in any prediction that can be made for geometrical contact and the difficulty of always determining what is geometrical contact in the observations, our formula for time of first contact is more than a sufficient one.

For first *internal* contact, it may be assumed that 21 minutes have to be added to the time of external contact at any place in these islands; while for the angle from N. point of first external contact may be taken in all cases 147°.

In the national ephemerides the times of the contacts are given for a particular meridian as they would be noted at the centre of the earth, and formulæ are appended to reduce these geocentric times to any point upon the earth's surface. It is obvious that where, as in a transit of Venus, predictions are required for such widely distant stations, this method possesses the greatest convenience.

NOTES

THE following are the Lecture arrangements at the Royal Institution for the ensuing Session:—The Christmas Lectures will be given by Prof. Tyndall, on Light and the Eye. Before Easter—Prof. W. C. Williamson on the Primeval Ancestors of Existing Vegetation, and their Bearing upon the Doctrine of Evolution; Prof. R. S. Ball, four lectures on the Supreme Discoveries in Astronomy; Prof. Dewar, nine lectures on the Spectroscope and its Applications; Mr. R. Bosworth Smith, four lectures on Episodes in the Life of Lord Lawrence; Dr. W. H. Stone, three lectures on Singing, Speaking, and Stammering; Mr. H. H. Statham, two lectures on Music as a Form of Artistic Expression. After Easter—Courses will be given by Professors Tyndall, McKendrick, A. Geikie, and Turner (of St. Petersburg). The Friday Evening Discourses will probably be given, among others, by Mr. R. B. Smith, Dr. G. J. Romanes, Sir W. Thomson, Mr. M. D. Conway, Prof. W. C. Williamson, Mr. W. H. Pollock, and Prof. Tyndall.

A COMMITTEE, consisting of the Right Hon. J. T. Ball, LL.D., D.C.L., the Very Rev. W. Reeves, D.D., Dean of Armagh, J. L. E. Dreyer, Ph.D., Astronomer of Armagh Observatory, has been appointed by the Governors of the Armagh Observatory to raise a fund for the purpose of erecting a memorial instrument in the observatory at Armagh, where the late Rev. Dr. Robinson spent fifty-eight years, engaged in those scientific investigations with which his name will be for ever associated. The Committee addresses its appeal not only to the inhabitants of Ulster, or of Dublin, but to Robinson's friends and admirers all over the United Kingdom. The services rendered to astronomy by Dr. Robinson are well known, and doubtless many of our readers will be glad to aid in paying a tribute to his memory. It is proposed that the memorial take the form of an equatoreal refractor, say of eight or nine inches aperture, which could be had for about 500*l.*, and could find room in one of the existing domes at Armagh. With such an instrument, much valuable and interesting work could be done. Subscriptions should be sent to Dr. J. L. E. Dreyer, Observatory, Armagh.

WE regret to announce the death, on November 24, of Mr. Andrew Pritchard, M.R.I., F.R.S. Edin., &c., of Highbury, London, whose name will be best remembered in connection with several improvements of the microscope, the use of "test objects," and as being the author of "A History of Infusoria," the fourth edition of which, enlarged to nearly 1000 pages, was published in 1861. Born in London in December, 1804, he was almost entirely brought up by his grandfather, one of the chief cashiers in the Bank of England. On the foundation of the Mechanics' Institution in Southampton Buildings, by Dr. Birkbeck, Mr. Pritchard entered as a student. The microscope was then a very imperfect instrument, and Mr. Pritchard worked hard at the achromatisation of lenses, and was the first to propose to take advantage of the high refracting power of the diamond, ruby, and sapphire for the manufacture of single lenses, these giving good definition without the coloured borders incidental to ordinary flint glass. Between the years 1829 and 1837 he published several works on the microscope, in which he was aided by Dr. Goring, particularly the "Microscopic Illustrations," "Micrographia," and the "Microscopic Cabinet," for which several good plates were prepared. In the year 1836 Mr. Pritchard was elected a Member of the Royal Institution, being proposed by Faraday, and in the previous year joined the British Association at Dublin, taking part in the deliberations of this body until comparatively recent times. In 1873 the Royal Society of Edinburgh conferred upon him their fellowship, in recognition of his scientific attainments, as evidenced by his great work, the "History of Infusoria," a memorial of patient industry and biological research.

THE Lancashire friends of the late Prof. Jevons are to hold a meeting at the Manchester and Salford Bank on Thursday next, to consider a proposal for a Jevons memorial. It has been suggested that an appropriate form of the memorial would be the establishment of a Professorship of Political Economy at the Victoria University, Manchester. Prof. Jevons was a Lancashire man, and was associated for many years with the Owens College and with the Manchester Statistical Society.

MR. BARNARD, of Nashville, Tennessee, and Prof. Wilson, of the Cincinnati Observatory, both noticed that the nucleus of the comet had separated into three fragments on the morning of October 5. While this separation was not observed at other observatories, probably owing to cloudy weather, we learn by the last steamer from Central America, that on the same morning the comet, as visible to the naked eye, at Escuintla, Guatemala, was divided into five distinct bodies, thus leading many to suppose that a whole family of celestial visitants had suddenly appeared. Subsequent observations in different parts of the world have led to the belief that the fragments were re-united. This statement appears in the *Panama Star and Herald*.

THE transit of Venus, on December 6, will be observed at Paris with the heliostat in several places, to exhibit the phenomenon to a large audience. M. Joubert, director of the Observatoire Populaire of the Trocadero, is taking steps for that purpose, and will send out special invitations. Lectures will be delivered during the transit. M. Janssen, before leaving for Oran, left instructions for similar observations to be exhibited before a number of visitors at Meudon Observatory. A requisition has been sent to M. Bouteiller, the president of the Municipal Council, asking him to order that the leading pupils of public schools and their principal teachers should be invited to Montsouris Observatory in order to witness the transit.

WE are glad to learn that Prof. Mendeleeff has published a new edition (the fourth) of his "Principles of Chemistry." The new edition is thoroughly revised, and contains many important additions and modifications, bringing it up to the latest data of science. The high standard of this book is well known. The aims the author has pursued may be seen in the following words of his preface: "By comparing the past of the science with the future, the particulars with the generalisations, and our necessarily limited experience with our natural tendency towards the infinite, and by refraining from asking the student to accept without test any doctrine, however attractive, I tried to develop in the reader the faculty of independent judgment on scientific subjects which is necessary for a true use of science, and for acquiring the possibility of working for its further development." The work may be regarded as not merely a text-book of chemistry, but an exposition of the methods of natural science altogether.

ALGERIA is becoming increasingly popular as a winter resort for invalids affected with chest disease; but probably not many of our readers are aware that in the same easily accessible country gout and rheumatic patients may find what is scarcely to be met with in Europe, a comfortable residence with abundance of waters adapted to their special complaints. At Hammam R'Irha, about sixty miles south-west of Algiers and fifteen miles in a direct line from the coast, such patients will find waters both for bathing and drinking comparable with those of the best European resorts, and in addition a climate which renders outdoor exercise a pleasure all the winter through. Hammam R'Irha is beautifully situated among the hills of an outlying spur of the Lesser Atlas, and we understand has every possible convenience and comfort that invalids can require. Naturally enough the people of Algiers look with some jealousy on this pleasant spot as a rival, and attempt we believe to ignore it; but in the opinion of the highest authorities on the subject of climate and waters, no place can equal Hammam R'Irha as a winter resort for gout and rheumatic patients. As it becomes better known we are sure it will grow in favour, especially with English and Americans, who will find on the spot competent medical advice. The station is within three or four hours' rail of Algiers.

THE remarkable phenomenon which was seen on Friday week in several parts of this country, was also seen in Sweden. At Eskibstuna, 54 miles south of Stockholm, it was observed three hours after sunset in the western heavens, it being dark at the time, about 45° above the horizon, and was then hidden in a lurid cloud of purple colour. When approaching the zenith an oblong object, somewhat resembling a bow, became distinctly visible, which gradually passed out of sight. The stars were visible through the object. The moon in her first quarter shone faintly in the south, 45° elevation above the horizon, while heavy clouds covered the eastern and northern skies. Auroræ were frequent and intense all over Scandinavia during the week.

HERR BERNHARD BLECHMANN, a pupil of Prof. Stieda, of Dorpat, has been making researches on the anthropology of the Jews. He took 100 Jews of West Russia and the Baltic Provinces, and as a result of his observations, he finds that there are both blonde and dark Jews of the primitive type, that Jews have narrower chests than Europeans under similar conditions; that there are two types, Spanish and Germano-Polish; and that they appear to be brachycephalic.

THE third German "Geographentag" will be held at Frankfort on March 29-31, 1883. As at former meetings, both the scientific and educational aspects of geography will receive attention, and intending contributors of papers should communicate with Prof. Rein, Marburg, before the end of January. There will, as usual, be an exhibition of teaching *matériel* in geography, which will be open for two or three weeks.

IMMENSE forest fires are reported from the neighbourhood of St. Petersburg. Near Pawlowsk and the villages Skolpino, Stepanowka, and Podberesche near Gatchina, then along the

Warsow railway line between Pljusse and Pleskau, also along the Moscow Railway line the forest was on fire. Thousands of people had been ordered out to try and extinguish the flames, but all attempts in this direction proved futile, the only thing that could be done was to confine the limits of the fires.

DR. KING, the Superintendent of the Royal Botanic Gardens, Calcutta, has recently issued his report for the year 1881–82. The Calcutta Garden may be said to be the centre of botanical work in India, and none can probably claim a greater antiquity, as the report before us is stated to be the ninety-fifth annual report of these Gardens. Like its predecessors the report opens with a description of the changes and improvements in the Garden itself, points which are, of course, only of local interest. On the subject of india-rubber yielding plants—a subject of very great importance—Dr. King says: "Clara rubber (*Manihot Glaziovii*) continues to grow well here; our trees are beginning to seed, and from their produce I was able to distribute during the year a good many seedlings to tea-planters in Assam, Chittagong, and elsewhere. A species of *Landolphia*, which is one of the sources of the rubber collected in Eastern Africa, has (thanks to the exertions of Sir John Kirk, Her Majesty's Consul-General at Zanzibar) been introduced to the Garden. From the seeds sent by Sir John Kirk a number of young plants have been raised, and these at present look very healthy. The cultivation of the plant yielding Para rubber (*Hevea brasiliensis*) has been abandoned, as the Bengal climate proves quite unsuitable for it. Of *Castilloa*, another South American rubber-yielder, we have as yet only eight plants, but it is being propagated as fast as possible." Another important subject is that of the production of materials for paper-making, and of these plantain fibre seems to have occupied some attention. It seems that during the dry months, simple exposure of the sliced stems to the sun is a sufficient preparation for the paper-maker, provided the paper-mill be on the spot. What is still wanted is some cheap mode of removing the useless cellular tissue, so that the fibre may be shipped to England without the risk of fermentation during the voyage. The cultivation of the plantain for its fruit is so universal over the warmer and damper parts of India, and its growth is so rapid, that the conversion into a marketable commodity of the stems at present thrown away as useless would be an appreciable addition to the wealth of the country. The paper mulberry of China and Japan (*Broussonetia papyrifera*) is being tried in the Garden, as well as in the Cinchona plantations in Sikkim, as it is well known that the bark yields a splendid paper material. A plant which appears to be at present unknown, but which Dr. King thinks will prove a species of *Eriophorum*, is also favourably reported upon. Under the head of "Other Economic Plants," mahogany, the rain-tree, and the Divi Divi, are said to be in considerable demand. A large interchange of seeds and plants has been effected during the year, with other parts of India, as well as with England and the Colonies.

No further news of the wreck alleged to have been seen near the Island of Waigatz has come to hand. Capt. Burmeister, of the *Louise*, who parted from the *Dijmphna* and the *Varna* in September last, is of opinion that the vessel seen is the *Varna* in her winter-quarters, simply with masts and yards lowered, which seem to be corroborated by the recent discovery, that the original message says *west* of Waigatz Island, where the wreck could not have drifted.

PARTS 11 to 16 of Dr. Chavanne's edition of Balbi's Geography (Vienna, Hartleben) have appeared; they are largely devoted to the Austro-Hungarian monarchy.

WE need scarcely mention that Oxford is the seat of the New Science Club, to the meeting at Trinity College in connection with which we referred last week.

IN the last sitting of the Syndicat d'Électricité M. Jablochkoff described a new element which he has invented, and which consists of sodium for the electro-positive plate, the negative being, as usual, carbon. M. Jablochkoff does not use any exciting liquid but merely sends into his elements by the instrumentality of an aspirator, a current of air saturated with moisture. He says that soda is dissolved and falls to the bottom of the box where his elements are kept so that it may be easily collected and sold at a high price, being pure except for a small quantity of carbonate and of nitrate. According to his statement the electromotive force of this element is about 4 volts.

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus* ♀) from West Africa, presented by Mrs. Gretton; a Northern Lynx (*Felis lynx*) from the Carpathian Mountains, presented by the Count Constantin Branicki; an Isabelline Lynx (*Felis isabellina*) from Tibet, presented by Capt. Baldock; a Forster's Milvago (*Milvago australis*) from Falkland Islands, presented by Dr. A. M. McAldonie; an Annulated Snake (*Leptodira annulata*) from Honduras, presented by Mr. R. E. Seabrooke; a Short-tailed Wallaby (*Halmaturus brachyurus* ♀) from West Australia, three Blue-crowned Hanging Parrakeets (*Loriculus galgulus*) from Ceylon, deposited; a Moloch Monkey (*Callithrix moloch*) from Brazil, two Snowy Owls (*Nyctea nivea* ♂ ♀), European, a Shore Lark (*Eremophila alpestris*), British, purchased; a Great Bustard (*Otis tarda*), European, received in exchange.

ON THE TRANSITS OF VENUS.[1]

TRANSITS of Venus usually occur in pairs; the two transits of a pair being separated by only eight years, but between the nearest transits of consecutive pairs more than a century elapses. We are now on the eve of the second transit of a pair, after which there will be no other till the twenty-first century of our era has dawned upon the earth, and the June flowers are blooming in 2004. When the last transit season occurred the intellectual world was awakening from the slumber of ages, and that wonderous scientific activity which led to our present advanced knowledge was just beginning. What will be the state of science when the next transit season arrives God only knows. Not even our children's children will live to take part in the astronomy of that day. As for ourselves, we have to do with the present, and it seems a fitting occasion for noticing briefly the scientific history of past transits, and the plans for observing the one soon to happen.

When the Ptolemaic theory of the solar system was in vogue, astronomers correctly believed Venus and Mercury to be situated between the Earth and the Sun, but as these planets were supposed to shine by their own light, there was no reason to anticipate that they would be visible during a transit, if indeed a transit should occur. Yet, singularly enough, so far back as April, 807, Mercury is recorded to have been seen as a dark spot upon the face of the Sun. We now know that it is much too small to be visible to the naked eye in that position, and the object observed could have been nothing else than a large sun-spot. Upon the establishment of the Copernican theory it was immediately perceived that transits of the inferior planets across the face of the Sun must occur, and the recognition of the value of transits of Venus for determining the solar parallax was not long in following. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks; but Halley was first to work it out completely, and to him is usually assigned the honour of the invention. His paper, published in 1716, was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus of 1761 and 1769, from which our first accurate knowledge of the Sun's distance was obtained.

When Kepler had finished his Rudolphine tables they furnished the means of predicting the places of the planets with some approach to accuracy; and in 1627 he announced that

[1] An address delivered before Section A of the American Association for the Advancement of Science, on August 23, 1882, by Prof. Wm. Harkness, Chairman of the Section, and Vice President of the Association.

Mercury would cross the face of the Sun on November 7, 1631, and Venus on December 6 of the same year. The intense interest with which Gassendi prepared to observe these transits can be imagined when it is remembered that hitherto no such phenomena had ever greeted mortal eyes. He was destitute of what would now be regarded as the commonest instruments. The invention of telescopes was only twenty years old, and a reasonably good clock had never been constructed. His observatory was situated in Paris, and its appliances were of the most primitive kind. By admitting the solar rays into a darkened room through a small round hole, an image of the Sun nine or ten inches in diameter was obtained upon a white screen. For the measurement of position angles a carefully divided circle was traced upon this screen, and the whole was so arranged that the circle could be made to coincide accurately with the image of the Sun. To determine the times of ingress and egress, an assistant was stationed outside with a large quadrant, and he was instructed to observe the altitude of the sun whenever Gassendi stamped upon the floor. Modern astronomical predictions can be trusted within a minute or two, but so great did the uncertainty of Kepler's tables seem to Gassendi that he began to watch for the expected transit of Mercury two whole days before the time set for its occurrence. On the 5th of November it rained, and on the 6th clouds covered the sky almost all day. The morning of the 7th broke, and yet there was no respite from the gloomy pall. Gassendi continued his weary watch with sickening dread that the transit might already be over. A little before eight o'clock the sun began to struggle through the clouds, but mist prevented any satisfactory observation for nearly another hour. Towards nine the sun became distinctly visible, and turning to its image on the screen, the astronomer observed a small black spot upon it. It was not half as large as he expected, and he could not believe it was Mercury. He took it for a sun-spot, and carefully estimated its position at nine o'clock, so that he might use it as a point of reference for the planet, if indeed he should be fortunate enough to witness the transit. A little later he was surprised to see the spot had moved. Although the motion was too rapid for an ordinary sun-spot, the small size of the object seemed to forbid the idea that it was Mercury. Besides, the predicted time of the transit had not yet arrived. Gassendi was still uncertain respecting the true nature of the phenomenon when the sun again burst through the clouds and it was apparent that the spot was steadily moving from its original position. All doubt vanished, and recognizing that the transit, so patiently watched for, was actually in progress, he stamped upon the floor as a signal for his assistant to note the sun's altitude. That faithless man, whose name has been forgotten by history, had deserted his post, and Gassendi continued his observations alone. Fortunately the assistant returned soon enough to aid in determining the instant of egress, and thus an important addition was made to our knowledge of the motions of the innermost planet of the solar system.

After this success in observing Mercury, Gassendi hoped he might be equally fortunate in observing the transit of Venus on December 6, 1631. He knew that Kepler had assigned a time near sunset for first contact, but the tables were not sufficiently exact to forbid the possibility of the whole transit being visible at Paris. Alas, alas! these hopes were doomed to disappointment. A severe storm of wind and rain prevailed on December 4th and 5th, and although the sun was visible at intervals on the 6th and 7th, not a trace of the planet could be seen. We now know that the transit happened in the night between the 6th and 7th, and was wholly invisible at Paris.

Transits of Venus can occur only in June and December, and as the two transits of a pair always happen in the same month, if we start from a June transit the intervals between consecutive transits will be 8 years, 105½ years, 8 years, 121½ years, 8 years, 105½ years, and so on. This is the order which exists now, and will continne for many centuries to come, but it is not always so. The path of Venus across the sun is not the same in the two transits of a pair. For a pair of June transits, the path at the second one is sensibly parallel to, and about twenty minutes north of, that at the first; while for a pair of December transits the parallelism still holds, but the path at the second one is about twenty-five minutes south of that at the first. Hence it happens that whenever Venus passes within about four minutes of the sun's centre at a June transit, or within about eight minutes at a December transit, she will pass just outside the sun's disk at the other transit of the pair, and it will fail. Thus the intervals between consecutive transits may be modified in various ways. If the first transit of a June pair fails, they will become 129½ years, 105½ years, 8 years, 129½ years, etc. If the second transit of a June pair fails, they will become 113½ years, 8 years, 121½ years, 113½ years, etc. If the first transit of a December pair fails, they will become 8 years, 113½ years, 121½ years, 8 years, etc. If the the second transit of a December pair fails, they will become 8 years, 105½ years, 129½ years, 8 years, etc. And finally, if either the first or second transit of a pair fails both in June and December, they will become 113½ years, 129½ years, 113½ years, 129½ years, etc.

When Kepler predicted the transit of 1631, he found from his tables that at her inferior conjunction on December 4, 1639, Venus would pass just south of the sun, and therefore he believed the second transit of the pair would fail. On the other hand, the tables of the Belgian astronomer, Lansberg, indicated that the northern part of the sun's disk would be traversed by the planet. In the fall of 1639 this discrepancy was investigated by Jeremiah Horrocks, a young curate only twenty years old, living in the obscure village of Hoole, fifteen miles north of Liverpool, and he found, apparently from his own observations, that although Kepler's tables were far more accurate than Lansberg's, the path of the planet would really be a little north of that assigned by Kepler, and a transit over the southern portion of the sun would occur. He communicated this discovery to his friend William Crabtree, and these two ardent astronomers were the only ones who had the good fortune to witness this, the first recorded transit of Venus.

Horrocks had great confidence in his corrected ephemeris of Venus, and it forbade him to expect the ingress of the planet upon the sun before three o'clock in the afternoon of Sunday, November 24, old style (December 4, new style); but as other astronomers assigned a date some hours earlier, he took the precaution to begin his observations on the 23rd. The 24th seems to have been partially cloudy, but he watched carefully from sunrise to nine o'clock; from a little before ten until noon; and at one o'clock in the afternoon; having been called away in the interval by business of the highest importance—persumably the celebration of divine service. About fifteen minutes past three he was again at liberty, and as the clouds had dispersed, he returned to his telescope and was rejoiced to find Venus upon the sun's disk, second contact having just happened. Only thirty-five minutes remained before sunset, but during these precious moments he made determinations of the position of Venus which are even yet of the highest value. Crabtree was less fortunate. At his station, near Manchester, there was but a momentary break in the clouds a quarter of an hour before sunset. This sufficed to give him a glimpse of the transit, and he afterwards made a sketch from memory.

The years sped swiftly by, and as the transit of 1761 approached, Halley's paper of 1716 was not forgotten, although he himself had long been gathered to his fathers. In deciding to what extent his plans could be followed, it was first of all necessary to know how nearly the real conditions would approximate to those he had anticipated. Passing over a paper by Trébucket calling attention to errors in Halley's data, Delisle was first to point out the exact conditions of the transit, and the circumstances upon which the sucess of the observations would depend. In August, 1760, less than a year before the event, he published a chart showing that inaccurate tables of Venus had misled Halley, both as to the availability of his method, and in the selections of stations. The occasion could be more effectively utilized by a change of plan, and Delisle considered it best to observe at suitably selected localities from many of which only the ingress, or only the egress, would be visible. Ferguson, in England, seems to have arrived independently at similar conclusions.

The two methods proposed respectively by Halley and Delisle have played so important a part in the history of physical astronomy that it will not be amiss to state briefly the distinction between them. The sun causes Venus to cast a shadow which has the form of a gigantic cone, its apex resting upon the planet, and its diameter continually increasing as it recedes into space. All the phenomena of transits are produced by the passage of this shadow cone over the earth, and as each point of the cone corresponds to a particular phase of a transit, any given phase will encounter the earth, and first become visible, at some point where the sun is just setting; and will leave the earth, and therefore be last visible, at some point where the sun is just rising. Between these two points it will traverse nearly half the earth's circumference and in so doing will consume about twenty minutes.

The only phases dealt with by either Halley's or Delisle's method are the external and internal contacts, both at ingress and at egress. Delisle's method consists in observing the times of contact at stations grouped about the regions where either ingress or egress is soonest and latest visible. The longitudes of the stations must be well determined, and then by combining them with the observed times of contact the rate at which the shadow cone sweeps over the earth becomes known, and from it the solar parallax results. At many of the stations best suited for Delisle's method, only the beginning or only the ending of the transit will be visible; but for the application of Halley's method, both the beginning and the ending must be seen. The theory of the latter method is so complicated that it is difficult to explain it briefly and at the same time accurately; but the the following considerations will suffice to indicate its nature. The duration of a transit at any point on the earth's surface depends partly upon the length of path, and partly upon the velocity, of that point while within the shadow cone. The length of path is affected by the latitude of the point, and the velocity by the earth's diurnal motion, which in some regions accelerates, and in others retards, the progress of the shadow. The result is that throughout one-half the earth's surface the duration of the transit is lengthened, while throughout the other half it is shortened; the maximum lengthening and shortening occurring at the respective poles of the hemispheres in question. Although these poles are not situated at the extremities of the earth's axis, it usually happens that one of them is shrouded in night; but upon the sunlit side of the earth, from which alone observations can be made, localities may exist at some of which the duration of the transit will be twenty minutes or more greater than at others. This inequality is the condition upon which Halley's method depends, and when such localities are accessible it may be advantageously applied. Briefly then, Halley's method consists in observing the duration of a transit at two or more stations so selected as to give durations of widely different lengths; while Delisle's method consists in employing a common standard time to note the instant when the transit begins, or ends, at two or more stations so chosen as to give very different values for that instant.

The transit of 1761 was visible throughout Europe and was well observed by astronomers in all parts of that continent. Besides this, England sent expeditions to St. Helena and to the Cape of Good Hope; and English astronomers observed at Madras and Calcutta; French astronomers were sent to Tobolsk, Rodriguez, and Pondicherry; Russians to the confines of Tartary and China; and Swedes to Lapland. No less than 117 stations were occupied by 176 observers; and of these, 137 published their observations. When this mass of data was submitted to computation, the result was far from satisfactory. Values of the solar parallax were obtained ranging from 8.49 seconds to 10.10 seconds; and in their disappointment the astronomers of the eighteenth century concluded that too much reliance had been placed upon Delisle's method.

The transit of 1769 drew on apace; and, to avoid a repetition of the fancied mistake of 1761, attention was directed almost exclusively to Halley's method. The conditions of the transit were carefully discussed by Hornsby in England, and by Lalande and Pingré in France; and it was found that its duration would be greatest in Lapland and Kamschatka, and least in the Pacific Ocean, California and Mexico. Astronomers were dispatched to all these regions. England sent the famous Capt. Cook to Otaheite, France sent Chappe to California; the King of Denmark sent Father Hell to Lapland; and in addition numerous observations were made in Europe, North America, China, and the East Indies. The preparations were most elaborate, and the result better than in 1761, but still not satisfactory. The black drop and other distortions disturbed the contacts in this transit as they had done in the previous one, and the values of the parallax deduced by the best computers ranged from 8.43 seconds to 8.85 seconds.

Thus the matter rested till 1825 and 1827 when Encke published abstracts of his discussion of the transits of 1761 and 1769, from which he deduced a parallax of 8.58 seconds. This discussion was not printed in full till 1835, when it immediately commanded the attention of astronomers, and its result, which Encke had modified to 8.57 seconds, was universally accepted for more than a quarter of a century. As time wore on, certain gravitational investigations led to a strong suspicion that the sun's distance had been over-estimated by at least three million miles, and the observations of Mars at its opposition in 1862 converted this suspicion into a conviction. The eighteenth century transits were again rediscussed and a parallax of 8.83 seconds was found from them by Powalky in 1865, and 8.91 seconds by Mr. E. J. Stone in 1868. Newcomb's paper, in 1867, also produced a marked impression.

The transit of 1874 was then approaching, and in the discussion as to how it should be utilized Halley's and Delisle's methods once more played a prominent part. It was recognized that the uncertainty in the observed times of contact of the eighteenth century transits was largely due to the black drop, and the causes of that phenomenon were carefully considered. Among them, most astronomers believed that irradiation played an important, if not the principal, part; but at the same time there was a general feeling that the telescopes of a century ago were bad, and that the magnificent instruments of the present day would give better results. In view of all the circumstances it was determined that the contacts should be observed with equatorially-mounted achromatic telescopes of from 4 to 6 inches' aperture or with reflectors of not less than 7 inches' aperture, and that magnifying powers of from 150 to 200 diameters should be employed. The Germans and Russians adopted heliometers of about three inches' aperture for making exact determinations of the positions of Venus during transit, but other nations did not follow their example.

Photography, an agency undreamed of in the eighteenth century, was also available, and all saw the desirability of employing it; but there was much difference of opinion as to how should this be done. The European astronomers preferred instruments modelled upon the Kew photoheliograph, whose objective has 3.4 inches aperture and 50 inches focus, giving an image of the sun 0.482 of an inch in diameter, which is enlarged by a secondary magnifier to 3.93 inches. On the other hand, the American astronomers contended that photographs taken with such instruments would be affected by troublesome errors due to the secondary magnifier, that position angles could not be measured from them accurately enough to be of any use, and that it would be exceedingly difficult to determine the exact linear value of a second of arc. They advocated the use of horizontal photoheliographs, which are free from all these disadvantages; and the instruments which they adopted had apertures of 5 inches, and focal distances of 38½ feet, giving images of the sun slightly more than 4 inches in diameter. Notwithstanding this radical difference of opinion respecting the best form of photoheliograph, the astronomers of the old and new worlds were in perfect accord as to how the instruments should be employed. Between the first and second contacts, and again between the third and fourth contacts, photographs about five minutes square, showing the indentation cut by the planet into the sun's limb, were to be taken at intervals of a few seconds; and from these it was hoped the true times of contact could be deduced with great accuracy. Between the second and third contacts, pictures of the entire sun were to be taken at short intervals, and the positions of Venus relatively to the sun's centre were to be obtained from them by subsequent measurements. In the latter case, the photoheliograph took the place of a heliometer, and was superior to that instrument in its power of rapidly accumulating data.

The question of instrumental outfit having been disposed of, stations were selected, and parties dispatched to almost every available point. The United States, England, France, Germany, Russia, Holland,—in short, nearly all the nations of the civilized world,—took part in the operations. The weather was not altogether propitious on the day of the transit, but nevertheless a mass of data was accumulated which will require years for its thorough discussion. When the parties returned home the contact observations were first attacked, but it was soon found that they were little better than those of the eighteenth century. The black drop, and the atmospheres of Venus and the Earth, had again produced a series of complicated phenomena, extending over many seconds of time, from among which it was extremely difficult to pick out the true contact. It was uncertain whether or not different observers had really recorded the same phase, and in every case that question had to be decided before the observations could be used. Thus it came about that within certain rather wide limits the resulting parallax was unavoidably dependent upon the judgment of the computer, and to that extent was mere guesswork. Attention was next directed to the photographs, and soon it began to be whispered about that those taken by European astronomers were a failure. Even yet I am not aware

that the Germans have published anything official on the subject; but the English official report has appeared, and it frankly declares that "after laborious measures and calculations it was thought best to abstain from publishing the results of the photographic measures as comparable with those deduced from telescopic view." From the way in which these photographs were taken, Sir George Airy saw that they could not yield position angles of any value, and therefore differences of right ascension and declination could not be determined from them; but they did seem capable of giving the distance between the centres of Venus and the sun with considerable accuracy. Upon trial this proved not to be the case. No two persons could measure them alike, because "however well the sun's limb on the photograph appeared to the naked eye to be defined, yet on applying to it a microscope it became indistinct and untraceable, and when the sharp wire of the micrometer was placed on it, it entirely disappeared." In short, the British photographs are useless for the present, but Sir George Airy hopes that in the future some astronomer may be found who will be capable of dealing with them.

We turn now to the American photographs. They present a well defined image of the sun about 4.4 inches in diameter, and are intended to give both the position angle and distance of Venus from the sun's centre. A special engine was at hand for measuring them, but when they were placed under the microscope only an indistinct blur could be seen. Here again was the same difficulty which had baffled the English, but fortunately its cause was soon discovered. The magnifying power of the microscope was only 37¼ diameters, which seemed moderate enough, but was it really so? The photographic image of the sun was about 4.4 inches in diameter, and this was magnified 3.31 times by the objective of the microscope, thus giving an image 14.56 inches in diameter. To yield an image of the same size, a telescopic objective would require a focus of about 1563 inches, and if the eye-piece of the microscope, which had an equivalent focus of 0.886 of an inch, were applied to it, a power of 1764 diameters would be produced. This then was the utterly preposterous power under which the image of the sun was seen when the photograph was viewed through the microscope, and no useful result could be expected from it. Means were immediately provided for reducing the power of the microscope to 5.41 diameters, and then the photograph seen through it appeared as the sun does when viewed through a telescope magnifying 255 diameters. After this change all difficulty vanished, and the photographs yielded excellent results. The measurements made upon them seem free from both constant and systematic errors, and the probable accidental error of a position of Venus depending upon two sets of readings made upon a single photograph is only 0.553 of a second of arc. To prevent misunderstanding it should be remarked that this statement applies only to pictures taken between second and third contact, and showing the entire sun. The small photographs taken between first and second contact and again between third and fourth contact, proved of no value.

These investigations consumed much time, and before the result from the American photographs was generally known, an international convention of astronomers was held in Paris to consider how the transit of 1882 should be observed. The United States was not represented at this conference, and guided only by their own experience, the European astronomers declared that photography was a failure and should not be tried again. They knew that the contact methods are attended by difficulties which have hitherto proved insurmountable, but under the merciless pressure of necessity, they decided to try them once more. Unfettered by the action of the Paris Conference, the United States Transit of Venus Commission took a very different view of the case. Its members knew that the probable error of a contact observation is 0.15 of a second of arc, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. They also knew that the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only 0.55 of a second of arc. If the sun is visible for so much as six minutes between the second and third contacts, by using dry plates thirty-six negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. These were the reasons which led the American Commission to regard photography as the most hopeful means of observation, and thus it happens that the astronomers of the old and new worlds differ radically respecting the best means of utilizing one of the most important astronomical events of the century. The Europeans condemn photography, and trust only to contacts and heliometers; the Americans observe contacts because it costs nothing to do so, but look to photography for the most valuable results

In 1716, Halley thought that by the application of his method to the transit of 1761, the solar parallax could certainly be a determined within the five hundredth part of its whole amount. Since then, three transits have come and gone, and the contact methods have failed to give half that accuracy. From the photographic method, as developed by the U. S. Transit of Venus Commission, we hope better things, and perhaps fifty years hence its results may be regarded as the most valuable of the present transit season. In 1874, as in 1761, exaggerated views prevailed respecting the value of transits of Venus, but no competent authority now supposes that the solar parallax can be settled by them alone. The masses of the Earth and Moon, the moon's parallactic inequality, the lunar equation of the earth, the constants of nutation and aberration, the velocity of light, and the light equation, must all be taken into account in determining the solar parallax, and it cannot be regarded as exactly known until the results obtained from trigonometrical, gravitational, and phototachymetrical methods are in perfect harmony. It may be be many years before this is attained, but meanwhile practical astronomy is not suffering. Its use of the solar parallax is mainly confined to the reduction of observations made at the surface of the earth to what they would have been if made at the Earth's centre; and for that, our present knowledge suffices. The real argument for expending so much money upon transits of Venus is that being an important factor in determining the solar parallax, their extreme rarity renders it unpardonable to neglect any opportunity of observing them. Let us do our whole duty in this matter that posterity may benefit by it, even as we have benefited by the labours of our predecessors.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The professoriate has been strengthened by the election of Dr. Burdon Sanderson to the new Chair of Physiology on the Waynflete foundation in connection with Magdalen College. The Biological side of the Museum will now be divided into two departments.

The Brakenbury Natural Science Scholarship at Balliol College has been awarded by the Examiners to Mr. Walker Overend, of the Yorkshire College of Science and St. Bartholomew's Hospital.

CAMBRIDGE.—Messrs. J. W. Hicks and F. Darwin are appointed members of the Botanic Garden Syndicate; Dr. Ferrers (Master of Caius), and Prof. Stuart, of the Museums and Lecture-Rooms Syndicate; Prof. Stuart is also specially re-appointed to the Local Examinations and Lectures Syndicate; Dr. Ferrers and Mr. Routh are appointed on the Observatory Syndicate; Prof. Humphry and Mr. Vines, on the State Medicine Syndicate; Mr. Trotter, on the Special Board for Medicine; Mr. Besant, on the Special Board for Mathematics; Mr. Shaw, on the Special Board for Physics and Chemistry; Mr. Vines, on the Special Board for Biology and Geology.

Messrs. J. C. Saunders and J. W. Hicks are approved as Teachers of Botany and Chemistry respectively for the purpose of certificates for Medical Students.

The following colleges have offered open exhibitions or scholarships for natural science, with examinations in December or January next: Trinity, examination, December 12, one exhibition of 50*l.* for two years; candidates to be under nineteen on March 25 next. St. Johns, one exhibition, 50*l.*, for three years, examination December 12; Caius, Jan. 8; Christ's, Emmanuel, and Sidney, January 12, a joint examination; candidates for all these must be under nineteen years of age. Particulars may be obtained from the tutors of the respective colleges.

GLASGOW.—The following appointments to Scholarships, &c., have been made in accordance with the results of the Competitive Examinations:—George A. Clark Scholarship in Mental Philosophy (£200 for four years), John S. McKenzie, M.A.; William Ewing Fellowship in Mental Philosophy (£80 for three years), James A. McCallum, M.A.; Eglinton Fellowship in

Mathematics and Natural Philosophy (£100 for three years), John Weir; James Ferguson Bursary in Mathematics (£70 for two years), William Weir; Breadalbane Scholarship in Mathematics (£50 for three years), James Hamilton, M.A.; John Clark (Mileend) Scholarship in Natural Science (£50 for four years), William Huntly, M.A.; John Clark (Mileend) Scholarship in Classics (£50 for four years), James McMillan.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 16.—On *Megalania prisca*, part 4, by Prof. Owen, F.R.S.—The author, referring to a remark during a discussion following a previous communication (April, 1880), on the great horned Saurian of Australia, viz. that the skull then described might have belonged to a Chelonian, not to the genus and species founded on fossil vertebræ from localities remote from the formation yielding the cranial evidence, proceeded to describe his latest received fossils from the river-bed in Darling Downs, which included, besides evidences of the pelvis and limbs of *Megalania*, also dorsal vertebræ identical in size and character with those on which the former existence of such large Saurian had been predicted (1858). The contiguity of the last discovered vertebra, by Mr. G. F. Bennett, to the cranial and caudal fossils previously found and transmitted by him to the author, and the absence of any remains of a Chelonian reptile in the whole extent of the dried up bed of the river so perseveringly explored by that gentleman, would permit no doubt, the author believed, as to the conclusions which had been admitted in the previous volumes of the *Philosophical Transactions*.

Linnean Society, November 16.—F. Crisp, LL.B., Treasurer, in the chair.—Mr. O. T. Olsen and Surgeon J. N. Stone, R.N., were elected Fellows.—Dr. W. C. Ondaatje exhibited and made remarks on some Ceylon plants, among these, the fruit of *Kandia dumetorum*, a remedy for dysentery; the leaves of *Sethia acuminata*, anthelmintic, and the resin of *Semecarpus gardneri*, from which a black varnish is prepared.—Mr. W. T. Thiselton Dyer called attention to a specimen of *Cycas beddomei*, a new species from Southern India.—Mr. F. P. Balkwill exhibited a series of British Foraminifera under the microscope, and said a few words on the special mode of mounting the same.—Mr. F. J. Hanbury showed a large fungus grown in a City wharf cellar, and which Mr. G. Murray pronounced to be a species of *Lentinus*.—Mr. C. Stewart exhibited a specimen of *Pilobolus*, explaining his observations on the projection of its sporangia.—Mr. J. G. Baker read a paper on the flora of Madagascar. It contains descriptions of 140 new species of poly petalous dicotyledons, obtained by the Rev. R. Baron and Dr. G. W. Parker. Some are of widely-diffused tropical genera, such as *Eugenia*, *Crotalaria*, &c.; others are of more familiar temperate types—*Alchemilla*, *Clematis*, and *Polygala*. Still others are characteristic of the Cape flora now noted for the first time from Madagascar, such as *Sphedamnocarpus* and *Sparmannia*. There is an interesting new genus of Malphigiaceæ (*Microsteira*) allied to the American *Hiræa*. Representatives of *Hibbertia* and *Rulingia* are interesting, from their being characteristically Australian genera. Mr. Baron has rediscovered *Rhodolæna alterola*, a showy plant, originally found by Du Petit Thouars a century ago. Dr. Parker has paid special attention to the drugs, esculents, and timber trees of the island, and catalogued 300 native names of the same.—A note by Mr. E. P Ramsay, on the type specimen of Finsch's Fruit Pigeon was read.—Dr. Maxwell Masters read a communication on the Passifloræ collected in Ecuador and New Granada by M. Ed. André. Of *Tacsonia* 9 species are mentioned; of *Passiflora* 29 species, four being new. Some are of special interest structurally, the excellence of the specimens enabling ample examination of the curious flower structure to be made.—A paper was read on cerebral homologies, by Prof. Owen. He compares the brain of the locust and the cuttle-fish with that of the fish, and other higher forms, and summarises as follows:—That the homologies of the primary divisions of the brain in molluscs are the parts known in Articulates as the supra- and sub-œsophageal ganglions, with their commissural cords. That the topical relations of these parts to the gullet are the same in both great divisions of invertebrates; and that the homologies of the aforesaid parts with the primary divisions of the vertebrate brain are affected solely by the altered relation thereto of the gullet and mouth.—A paper was read on *Cassia lignea*, by W. T. Thiselton Dyer.—Thereafter, the sixteenth contribution to the mollusca of the *Challenger* Expedition by the Rev. R. Boog Watson, was read, in which were described the families Fissurellidæ and Cocculinidæ.

Chemical Society, November 16.—Prof. Dewar, vice-president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting (December 7).—The following communications were made:—Contributions to the chemistry of tartaric and citric acids, by the late B. J. Grosjean. This paper has been compiled by Mr. Warington from the laboratory note-books left by the author. It contains investigations as to the loss of water by different specimens of citric acid, the determination of citric acid in lemon, bergamot, lime, and orange juices, the influence of heat on solutions of a totartaric acid, influence of sulphuric acid on the crystallisation of tartaric acid, action of solutions of potassium and sodium sulphates on calcium tartrate, determination of free sulphuric acid in tartaric acid liquors, determination of tartaric acid by precipitation as bitartrate, &c.—Contributions to the chemistry of bass fibres, by C. F. Cross and E. J. Bevan. The authors detail further experiments, showi g that lignified fibres are to be regarded as a chemical whole rather than the mixture which was necessitated, viz. the incrustation theory.—On the oxidation of cellulose, by C. F. Cross and E. J Bevan. By the action of boiling 60 per cent. nitric acid, cellulose is converted into an amorphous substance $C_{18}H_{26}O_{16}$, oxycellulose.—On the analysis of certain vegetable fibres, *Annanassa*, *Musa*, *Phormium*, *Linum*, *Urtica*, &c., by C. S. Webster.—On the constitution of some bromine derivatives of naphthalene (third notice), by R. Meldola. The author concludes that Glaser's α dibromnaphthalene and Meldola's metadibromnaphthalen are isomeric, and not identical. The author has also obtained by the diazo reaction β dibromnaphthalene, m.p. 81°, a new tribromnaphthalen, m.p. 113–114, a second melting at 63°, &c.—On the constitution of lophin (second notice), by F. R. Japp. The author brings forward fresh proofs that this body has the constitution of an anhydrobase, and not that ascribed to it by Radziszewski.

Geological Society, November 15.—Dr. J. Gwyn Jeffreys, F.R.S., vice-president, in the chair.—John Edmund Thomas and Joseph Williams were elected Fellows of the Society.—The following communications were read:—The drift-beds of the North-west of England and North Wales; part 2, their nature, stratigraphy, and distribution, by T. Mellard Reade, C.E., F.G.S. The author stated that the first part of this paper, read in 1873, treated of the low-level boulder clay and sands, specially in relation to the contained shells. Since that time he has been diligently collecting information to enable him to treat of the nature, origin, and stratigraphy of the drift lying between Liverpool and St. Bees and Liverpool and Carnarvonshire. The author's conclusions are that an ice-sheet, radiating from the mountain district of the English lakes and the south of Scotland, produced the planing and grooving of the rock and the red sand and rubble *débris*; then the ice melted back into local glaciers, and the submergence began. The low-level boulder-clay and sands were, during a slow submergence, laid down probably at depths of from 200 to 300 feet, and the author considers that all the phenomena can be satisfactorily accounted for by ordinary river-action and fraying of the coasts by the sea, combined with frost and ice due to a severer climate bringing down the materials of such river-basins to the sea, while icebergs and coast-ice sailed over, dropping on the sea-bottom their burdens of erratic stones and other materials from the mountain-districts of the north. He pointed out, also, that the great majority of the well-glaciated rocks were specially those that could be traced to the high lands. This fact was forced upon his notice after making a large collection of glaciated boulders and pebbles. Among the rocks he had been able to identify, with the help of Prof. Bonney and Mr. P. Dudgeon, of Dumfries, Scawfell granite (Eskdale, of Mackintosh) was the most abundant granite; then came grey granites from Dumfries, syenite from Buttermere, which occurred all over the area described, and up to 1200 feet on the Macclesfield Hills, and syenite from Cannockfell. Other probable identifications were also named. The whole series of rocks from the Silurian to the New Red Marl were represented in the low-level boulder-clay; a few flints also occurred, and one piece of what was believed to be chalk. The paper concluded with an appendix by Mr. David Robertson,

giving a list of the Foraminifera and other organisms found in the various beds of boulder-clay in the Atlantic Docks, Liverpool.—On the evidences of glacial action in South Brecknockshire and East Glamorganshire, by Mr. T. W. Edgeworth Davy. Communicated by Prof. J. Prestwich, F.R.S. The area which is included in this paper is about 200 square miles, extending north and south from the Brecknockshire Beacons to a line between Cowbridge and the mouth of the Rhymney, of which the Cly valley has been more particularly studied. Most of the rocks in this district, and particularly the Millstone Grit, retain traces of glacial markings. The whole area has a *moutonnée* aspect.

Anthropological Institute, November 14.—Mr. Hyde Clarke, vice-president, in the chair.—Mr. R. W. Felkin exhibited a Darfur boy who was rescued from slavery and brought to England by him in 1879.—Mr. Francis Galton exhibited a box about the size of a backgammon board, containing a geometric series of test weights for comparing the sensitivity of different persons. The test lay in ascertaining the smallest difference that could be perceived when handling them. The lowest weight was 1000 grains, which gives no uncertain sense of heaviness, and the highest weight was far short of what would fatigue the hand. Consequently, by Weber's law, the difference in the sense of heaviness produced by handling any two of the weights is the same, or nearly so, whenever those weights are separated by the same number of terms. For example, a person who could just and only just distinguish between the 4th and the 8th weight would do the same as regards the 2nd and the 6th, and the 6th and the 10th, the number of terms interval being 4 in each case. Again, as the only interpretation possible to the phrase that "the sensitivity of A is *r* times as great as that of B," is that A can perceive *r* grades of difference when B can only perceive one, it follows that the relative sensitivity of two persons is inversely proportionate to the number of terms between any pair of weights that they can respectively just discriminate. The unit of ratio was 2 per cent., but in the earlier terms there was a sequence of half units. The weights were made exactly alike in outward appearance; they were common gun-cartridges, stuffed equably with shot and wads and closed in the usual way. The term in the series to which each weight belonged was written on the wad that closed it. It was shown that the best economy of time was to present the weights in threes, to be sorted in their proper order. By making a proper selection, a wide range of testing power could be obtained by 30 cartridges ranged in 10 trays. The same principle admits of being extended to testing the delicacy of other senses, as taste and smell. Some provisional results were mentioned: (1) that intellectually able men had, on the whole, high powers of discrimination; (2) that men had more discriminating power than women; (3) that highly sensitive women did not seem able to discriminate more grades than others, though both sensation and pain were induced in them by lower stimuli; (4) that the blind, as a whole, were not peculiarly sensitive to this test, but rather the reverse. A discussion followed, in which Prof. Croom Robertson, Dr. Camps, Mr. Sully, D. Mortimer Granville, Dr. Mahomed, Mr. C. Roberts, Prof. Thane, and others took part.

Royal Horticultural Society, November 14.—Dr. M. T. Masters in the chair.—*Proliferous Flowers:* The Rev. G. Henslow exhibited a *Rhododendron balsamiflorum aureum*, with flowers arising from the centre of the pistil. The latter organ had dehisced longitudinally, and a cluster of malformed orange-coloured petals protruded from the orifice. He observed that every flower on one bush in his garden, of a common pink kind, had, during the last season, formed a blossom within the pistil, though in the latter case the flowers so formed had perfect as well as petaloid stamens. In every case the flower sprang from the axis at the base of the ovary. *Carnation:* a blossom with a secondary flower arising from within the calyx. *Blue-bell:* Each flower was borne on a pedicel of about two inches in length, and produced a secondary flower from the axil of a perianth leaf. In the place of one flower a complete raceme had grown.—*Solomon's Seal:* Leafy racemes occupied the positions of normal flowers.—*Monstrous Flowers:* He also showed the following:—*Pistillody of calyx in Violets*, in which the organs were in part or entirely virescent and malformed, having the sepals abortively ovuliferous, and the petals often laciniated. The sepals bore papiliform structures on the margins and mid-ribs, resembling rudimentary ovules. The only recorded instance of ovuliferous sepals was that of the garden pea, figured and described in the *Gard. Chron.* 1866, p. 897. *Pistillody of stamens:* He showed drawings illustrating various stages of ovuliferous stamens of the Alpine poppy. *Syngenesism* in *Diplotaxis tenuifolia:* The anthers of every flower cohered laterally, so that the pollen could not escape; the consequence being that in no case did a flower set seed. *Placental protrusion* in *Begonia:* Portions of the placentas covered with ovules had protruded externally from the summit of the ovary, apparently being due to hypertrophy.—*Chrysanthemum:* Dr. Masters showed a blossom, half the florets being white, the other half yellow, a diameter separating the two kinds.

CAMBRIDGE

Philosophical Society, November 13.—On the structure of the spleen, by Mr. J. N. Langley.—On the continuity of the protoplasm in the motile organs of leaves.—Dr. W. H. Gaskell exhibited two pieces of muscular tissue from the ventricle of a tortoise, one of which had been taught to execute rhythmical contractions.

PARIS

Academy of Sciences, November 20.—M. Jamin in the chair.—A letter was read from the Minister of Public Instruction, giving an *arrêté* which fixes the conditions of the next Volta prize, to be awarded in 1887 (see p. 89).—Results of experiments made at the Exhibition of Electricity on incandescent lamps, by MM. Allard and others. In general and for the spherical mean intensity of 1·20 Carcel, only about 12 to 13 Carcels per h.p. of arc, or 10 Carcels per h.p. of mechanical work, can be counted on, from incandescent lamps. Electric candles give 40 Carcels per h.p. of arc, regulators nearly 100, so that, generally, the economic values of the three systems are nearly as 1, 3, and 7.—Researches on the iodide of lead, by M. Berthelot.—On the decomposition of cyanogen, by the same.—Researches relative to the vision of colour, by M. Chevreul.—On the relation of lunar to solar action in the phenomena of the tides, by M. Hall.—Chemical studies on Silesian beet (continued), by M. Leplay.—Electro-chemical deposits of various colours, produced on precious metals, for jewellery, by M. Weil. He presented pieces of gold and silver jewellery, polychromised industrially with oxides of copper, by his processes. The colours resist friction, dry or moist air, air vitiated by sulphuretted hydrogen or coal gas, and light. M. Edm. Becquerel recalled the colorations obtained by his father with oxides of lead and iron.—On a *sulphocarbometer* for determining the quantities of sulphide of carbon contained in alkaline sulphocarbonates, by MM. Gélis. A glass flask filled with a solution of bisulphite of soda or potash, has on its neck a metallic sleeve with internal screw-thread; into this is screwed a corresponding metallic tube with stopcock under the terminal bulb of a graduated and closed glass tube holding the sulphocarbonate to be examined. On opening the cock, the liquids mix. The reaction is completed when the sulphocarbonate becomes quite colourless; then the height of the column of sulphide of carbon is noted, and the weight of the sulphide of carbon that was in the sulphocarbonate may be deduced.—Results of treatment adopted in Switzerland with a view to destruction of Phylloxera, by M. Mayet (see p. 89).—On two standards of the *aune* and the *pied de Roi* recently found by M. Wolf. He found them in the maritime arsenal of Cherbourg. They are at present the sole representatives of an attempt at unification of French measures long before the birth of the metric system, and the only models of ancient measures preserved in their integrity.—Observations of the planet (216) Cleopatra, and of the great comet of 1882, at Paris Observatory (equatorial of the western tower), by M. Bigourdan.—Observations of the same comet at Algiers Observatory, by M. Trépied.—On the same, by M. Jaubert. He notes (from the Popular Observatory) that the central part, or true tail, had a paler envelope, which nearly ceased to be visible as the comet rose above the horizon and the tail shortened—except a part nearest a Hydræ, which seemed brighter than at first.—On the solar energy, by M. Rey de Morande. The great uniformity of terrestrial vegetation till the Cenomanian epoch, and gradual differentiation since, according to latitude, the gradual invasion of southern regions by trees with caducous leaves, and disappearance of all vegetation in Polar regions, are phenomena explicable by gradual contraction of the sun, but inexplicable by gradual cooling of the earth.—On the works of Frederic Houtman, by

M. Veth.—On the functions of a single variable similar to the polynômes of Legendre, by M. Hugoniot.—On the motion of a system of two electrified particles of ponderable matter, and on the integration of a class of equations with partial derivatives, by M. Lévy.—Production by the dry way of some crystallised uranates, by M. Ditte.—On the second anhydride of mannite, by M. Fouconnier.—Action of tri-ethylamine on symmetrical trichlorhydrine, and on the two isomeric dichlorhydric glycides, by M. Reboul.—Note on the study of *longrain*, and the measures of schistosity in schistous rocks by means of their thermic properties, by M. Jannettaz. The large axis of the isothermal surface is parallel to the *longrain* or second cleavage, and the small axis is perpendicular to the first cleavage or plane of schistosity.—Lithine, strontian, and boric acid in the mineral waters of Contrexeville and of Schinzmark (Switzerland), by M. Dieulafait.—Experiments on the calcination of alunite in powder, for manufacture of alum and sulphate of alumina, by M. Guyot.—On the anastomoses of striated muscular fibres in invertebrates, by M. Jousset de Bellesme. They insure simultaneous contraction (*e.g.* in gastric glands).—On the functions of the digitiform or superanal gland of Plagiostomes, by M. Blanchard. It appears to be a true pancreas.—Evolution of the epithelium of poison glands in the toad, by M. Calmels.—On two tertiary *Plagiaulax*, obtained in the neighbourhood of Rheims, by M. Lemoine.—On the *Tingis* of the pear-tree, by M. Carlet.—Some letters on the recent aurora were communicated.

BERLIN

Physiological Society, November 10.—Prof. Du Bois-Reymond in the chair.—Dr. J. Geppert gave an account of some experiments which he and Dr. A. Fraenkel had made on the effect of rarefied air on the organism, in order to test the statements which Prof. Paul Bert had made on respiration in rarefied air, and the accompanying deficiency of oxygen in the blood. The animal experimented on—a dog—was unfettered in a box of sheet iron, with a glass window, in which it was possible to produce every desirable pressure with an air-pump, and the ventilation could easily be accomplished during the attained rarefaction. If the gas pressure in the box was sinking, by degrees, the animal did not show any change in its behaviour or its functions until the pressure was reduced to 38 cm. Then and at a further lessening of the pressure, the animal became restless, the respiration grew deeper and faster; again, at a further rarefaction of air, the movements became uncertain and giddy; at a pressure of 25 cm., one-third atmosphere, the animal fell asleep like a normal sleeping animal, and could remain so six or seven hours without any hindrance to his later complete restoration; occasionally awakened, the animal had severe paroxysms of dyspnœa, which, however, soon passed, and it fell again fast asleep. By further rarefaction, nearly to 15 cm., severe paroxysms of dyspnoea and convulsions very soon caused the death. Nearly the same appearance and the same succession of phenomena aëronauts have described during balloon ascensions: in the first stage a quite normal feeling, then accelerated and deeper respiration, faintness of the limbs, which increased to paralysis; during the increasing inability of moving the voluntary muscles, drowsiness begins, from which there was no awakening if the balloon still rises to more rarefied regions, as was the case with the unhappy aëronauts Cravé, Spinelli, and Sivel. These symptoms differ in no way from the phenomena described in these experiments, but the stages begin much sooner, and the 25 cm. pressure at which the dog only fell into a deep sleep is the extreme limit of available rarefaction for the aëronaut. The cause of this more early beginning of the stages of disease may be first the low temperature of the higher regions which Mr. Glaisher showed to sink till $-20°$, and otherwise the continuous muscular activity causing stronger effects of the lower degree of rarefaction. Quite another appearance is presented by the mountain disease which is characterised by nausea, choking, and vomiting, besides the strong respiratory movements and increased heart's action. Dr. Geppert supposes that neither the often but moderate degree of rarefaction (60 cm. pressure) nor the trifling want of oxygen is the *vera causa*, but, as Mr. Dufour has already asserted, the extreme weariness of the body. (In the discussion which followed Dr. Geppert's communication, Prof. Du Bois-Reymond observed that some years ago he had advanced and proved the theory that the mountain disease, especially the vomiting on ascending high mountains, was a reflex phenomenon due to the very strong dazzling of the eyes by the vast intensely white and brightly insolated snow-fields.) Again, Dr. Geppert has made many measurements on the absorption of the oxygen by the arterial blood at varying gas tensions. The manner of blood-letting, the measuring of its volume, and the gas analysis, were much exacter and less objectionable than in the corresponding experiments of Mr. Bert, particularly for the measuring of the blood volume, an ingenious apparatus was used. The results of these experiments were that the proportion of oxygen in the arterial blood remained normal with decreasing oxygen tension, till the gas pressure was sunk to 40 cm. At further sinking of gas-pressure the proportion of oxygen in the blood decreased, and the deficiency of oxygen grew very considerable. Finally Dr. Gepper concluded that in the action of rarefied air on the proportion of the oxygen in the blood the physical absorption plays not so much a part as rather the chemical affinity for hæmoglobin for the oxygen.—Prof. Munk then reported briefly on experiments executed by Mr. Orshansky in his laboratory on the influence of anemia on the electric excitability of the brain; the anemia was produced by pouring the blood out of the femoral artery, and the excitability was tested in that part of the brain-surface which is the centre of motion of the fore and hinder legs, partly with the constant, partly with the inductive stream. In the first stages of the bleeding there was no change of excitability, then it increased, till about one-seventh of the total blood volume was poured out, then any further loss of blood continuously decreased the excitability, till finally, when about two-thirds of the blood was gone it sunk to zero. In every stage of anemia the maximum of corresponding change of excitability never appeared immediately, but some time after bleeding. Through all the changing of stages of excitability, except when the irritability was sunk to zero, re-creation and return to the normal state took place after an interval. No certain relation between the blood-pressure after the bleeding, and the rate of irritability corresponding to that state of anemia could be established.

VIENNA

Imperial Academy of Sciences, November 2.—T. Horbaczewski, on the synthesis of uric acid.—V. Patelt, on the development of the mucous membrane of the large intestine.—A. Tarolimek, on the relation between tension and temperature of saturated vapours and saturated carbonic acid.—E. Weiss, computations of the positions of the cometary nebulæ discovered by T. J. Schmidt, of Athens, on October 9.—Hr. Weidel and K. Hazura, on cinchonine.—R. Wegacheider, on isovanilline.—T. v. Oppolzer, on the criterion relating to the existence of three solutions of the cometic problem.—T. Wiesner, studies on withering leaves and leaf-shoot, a contribution to the knowledge of the transplantation of plants.

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